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THE CONDOR

A Magazine of Western Ornithology

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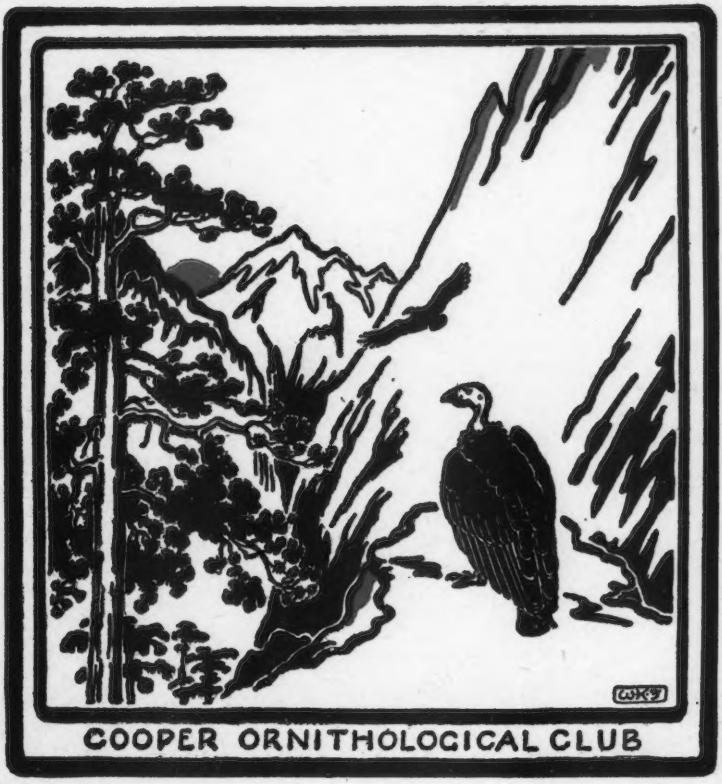

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CONTENTS

PAGE

Are Rings of Holes in Tree Bark Made by Downy Woodpeckers?.....*Charles W. Townsend* 61
Observations on the Head Markings of the Golden-crowned Sparrow (with one illustration).....*Joseph Mailliard* 66
Nesting of the Hammond Flycatcher in Eldorado County, California (with two photographs by Milton S. Ray).....*Rose Carolyn Ray* 71
Technique of Raptor Food Habits Study (with two illustrations).....*Paul L. Errington* 75
The Two Check-lists of 1931—A Critical Commentary.....*Joseph Grinnell* 87
The Summer Distribution of Certain Birds in Central and Northern Arizona.....*Alden H. Miller* 96

FROM FIELD AND STUDY

The Black Swift in Glacier National Park.....*Winton Weydemeyer* 100
A Station of Frequent Observation of the Cowbird in the San Francisco Bay Region.....*Gayle Pickwell* 100
Note on the Food of an Arizona Spotted Owl.....*Laurence M. Huey* 100
Bird Notes from the Upper Rio Grande Valley.....*James Stevenson* 101
The Wood Duck in Napa County, California.....*E. L. Bickford* 101

EDITORIAL NOTES AND NEWS..... 102

CURRENT DISCUSSION: Game and Wild Life Conservation.....*Aldo Leopold* 103

PUBLICATIONS REVIEWED:

Pickwell on the Prairie Horned Lark.....*T. T. McCabe* 106
Rowan on the Riddle of Migration.....*Alden H. Miller* 107

MINUTES OF COOPER CLUB MEETINGS..... 109

Report on Conservation.....*Jean M. Linsdale* 111

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ARE RINGS OF HOLES IN TREE BARK MADE BY DOWNY WOODPECKERS?

By CHARLES W. TOWNSEND

The late Mr. E. H. Forbush published in his "Useful Birds and their Protection" (1907) and in his "Birds of Massachusetts" (vol. II, 1927) certain evidence tending to show that the circular rings of holes, found especially in apple trees in middle and southern New England where the Yellow-bellied Sapsucker (*Sphyrapicus varius varius*) is merely a migrant, are made by the Downy Woodpeckers (*Dryobates pubescens medianus*), here resident birds. Similar conclusions were arrived at by Grinnell and Storer in their "Animal Life in the Yosemite" (1924) in which they attributed rings of holes in apple trees to the resident Willow Woodpeckers (*Dryobates pubescens turati*) and not to the migrant Red-breasted Sapsucker (*Sphyrapicus varius daggetti*).

First let us take up the evidence presented by Mr. Forbush. He quotes the older ornithologists, Wilson and Nuttall. They believed that the circular rings of holes seen so commonly in orchards in middle and southern New England and in the Middle States were made by Downy and, occasionally, by Hairy woodpeckers; but it is important to bear in mind that the older ornithologists did not know that the Yellow-bellied Woodpecker, as they called it, was a sapsucker and made peculiar drill patterns in living bark. Nuttall, in referring to the drills, says: "These perforations, made by our *Sap-Suckers*, as the present and preceding species [Downy and Hairy] are sometimes called, are carried round the trunks and branches of the orchard trees in regular circles . . . ".

In Samuels' "Birds of New England", formerly the universal reference book here, first published in 1867, the author quotes in full Wilson's detailed account of the rings of holes which he naturally assumes were made by the Downy Woodpecker, the resident species, as he has no knowledge of the habits of the true sapsucker in this regard. It is evident, therefore, that most New Englanders were brought up with this belief.

Coues, on the other hand, in his famous "Key", first published in 1872, says of *Sphyrapicus*: "This is the true sapsucker, which injures the orchardist, and brings the beneficial species of Dryobates into disrepute." Baird, Brewer and Ridgway in their "North American Land Birds", published in 1874, say of girdling and sapsuck-

ing by *Sphyrapicus*: "These habits, so well known to most of our Western farmers, appear to have entirely escaped the notice of our old ornithologists." Finally, Bendire, 1895, writing also of these habits of the latter bird, says: "Indirectly it also causes the death of many a Hairy and Downy Woodpecker (the best friends the fruit grower has), these species being frequently shot through ignorance of their habits or because they are mistaken for Sapsuckers." These references from Coues, Baird, Brewer and Ridgway, and Bendire are not quoted by Forbush, but, on the other hand, he quotes J. A. Allen as asserting that these rings of holes "are chargeable also to Downy and Hairy Woodpeckers."

Mr. Forbush does not give any observations of his own on this matter, and, in the "Birds of Massachusetts", he modifies a positive statement previously made in "Useful Birds and their Protection" as to the authorship of these numerous rings of holes in apple trees by the following: "I had seen the bird [Downy Woodpecker] apparently working at these holes in a region where thousands of such perforations could be seen . . . but I could not be certain that the Downy was the original maker of them."

Although a few of the observations from correspondents quoted by Mr. Forbush are positive, many leave one in considerable uncertainty as to whether the correspondents actually saw the Downy Woodpecker making the rings of holes, or merely tapping in the same region, or drinking the sap, or eating cambium from holes whose origin was not ascertained. It may be that some of the correspondents were unable to distinguish the true species of woodpecker.

There is one observation, however, which should be quoted here, as it is of considerable interest in this discussion, an observation made by a capable observer with great care. Forbush says, *loc. cit.*, vol. II, p. 268: "The first trustworthy evidence, however, that I obtained regarding the tapping of trees for sap by the Downy Woodpecker was in 1899, when my assistant, the late Charles E. Bailey, on April 6 watched one for several hours. His report reads: 'At 12:30 I found a Downy Woodpecker, and watched him till 2:45; he took three larvae from a maple stub, just under the bark. He next tapped two small swamp maples, four and six feet from the ground, and spent most of the time taking sap. He tapped the tree by picking it a few times very lightly; it looked like a slight cut, slanting a little. The bird would sit and peck the sap out of the lower part of the cut. The cut was so small the sap did not collect very fast. The bird would go and sit for a long time in a large tree and not move, then it would come back and take more sap. It did this three times while I was watching it. It did not care to take any food but the sap.' . . . Mr. Bailey cut off and brought me the limb, the bark of which was perforated by the bird. . . . The perforations passed through the bark to the wood, but did not enter it and they do not in the least resemble in shape those made by the Yellow-bellied Sapsucker." Here is just what we should expect in a woodpecker not specialized as a sapsucker.

Grinnell and Storer, "Animal Life in the Yosemite", 1924, found, as in eastern apple orchards, many circles and rows of holes in the trees of an orchard and they found a pair of Willow Woodpeckers, a western subspecies of the Downy, regular tenants there. They say, "We watched a bird at work; moreover bits of inner bark fibres were found adhering to the bristles around the bill of a bird shot" and they conclude that this pair "or their ancestors had evidently worked there for some years with the result that most of the one hundred and fifty trees in the orchard showed marks of their attention."

Owing to criticism of these conclusions by Mr. and Mrs. Charles W. Michael, who for many years have worked as nature guides in the Yosemite, Dr. Grinnell pub-

lished (Condor, 30, 1928, pp. 253-254) an article in which he reviews the notes previously made by Dr. Storer and himself and states, "The facts are precisely as recorded, the chief of which are that two birds of the latter species [Willow Woodpecker] were watched at certain fresh pits. One of these birds shot subsequently the same day, as per my notes, showed 'bits of inner applebark adhering to bristles around base of bill, showing [seemingly to me at the time] that he had excavated the pits'."

Dr. Grinnell quotes from Mr. Michael's letters: "For several years now the apple orchard has been under observation, and during this time many Red-breasted Sapsuckers have been seen drilling sap-pits, but never once have we seen Willow Woodpeckers doing work of this sort." They once watched for thirteen consecutive days a single sapsucker at work drilling holes and they believe that the observation made by Grinnell and Storer was of Willow Woodpeckers "merely looting the honey-pots of a Red-breasted Sapsucker and that they themselves had done no actual work in excavating the pits"; and Mr. Michael concludes with the shrewd remark: "Months go by when no sapsuckers are present, but there is never a month when Willow Woodpeckers are not present. It is hard to believe that Willow Woodpeckers drill holes in the bark only when there is a Red-breasted Sapsucker present in the orchard!" And he adds, "I think that inferences made by Grinnell and Storer were absolutely wrong."

All of this in a spirit of scientific fairness is published by Dr. Grinnell, who calls for further close observation and asks, "Were or were not the inferences made by Grinnell and Storer likely to have been 'absolutely wrong'?"

Before giving my own observations in Massachusetts it may be well to consider the theoretical view of the matter, although it is always to be remembered that theoretical objections are often overcome in nature and that it is well not to be too dogmatic. Actual unbiased observation should solve the problem. Bearing this in mind and not allowing these theoretical considerations to influence our conclusions unduly, several facts may be stated. In the first place, the genus *Sphyrapicus* has evolved as a specialized sapsucker. Its tongue differs from that of *Dryobates* and other woodpeckers in that it can be extended only a third of an inch beyond the bill, while the tongues of other woodpeckers are very extensile for the purpose of drawing insects out of burrows. The short tongue of the sapsucker is also provided with brush-like fringes so that it answers well the purpose of a sapsucker, but is not adapted to drawing out insects from deep burrows.

Careful and thorough studies such as those of Frank Bolles (Auk, 8, 1891, pp. 256-270) showed clearly that sap is the chief object of *Sphyrapicus* and its most important food in spring and early summer. The inner bark or cambium is also eaten, as well as insects attracted to the sap. Bolles found that other birds such as hummingbirds, chickadees and Downy Woodpeckers, take advantage of this food supply and drink of the sap; but this is a far different matter from the actual drilling of holes in the characteristic patterns of the sapsucker in order to obtain the sap. There is no reason why a Downy Woodpecker might not make some irregular cuts, as noted by Mr. Forbush, for the sap, or even drill a hole or two for the purpose, but we should not expect it to adopt the ring, or gridiron pattern of the sapsucker, an instinct undoubtedly of long duration in the latter bird. Actual observation would be necessary to prove this.

Many birds, when circumstances arise, eat various foods outside of their especial dietary, just as many birds depart from their usual custom of picking up insects on bark and leaves by at times catching them in the air like flycatchers. This is com-

monly seen among the warblers. The Cedar Waxwing is a well known, although awkward flycatcher, and even Yellow-bellied Sapsuckers at times indulge in the gentle art of flycatching. It is natural that a bird that is chasing an insect on the ground or in a tree should follow in pursuit when the insect takes to the air, or it even might dart out from a tree after a flying insect. The difference in habit is slight and not fundamental, but we would not expect an Eave Swallow to excavate a hole in a bank for a nest, or a Bank Swallow to build a retort-shaped nest of mud, or any but a sapsucker to make circles or vertical patterns of holes in the bark of trees for the purpose of obtaining the sap.

As to my own observations, I may state that, although I have long watched Downy Woodpeckers gleaning insects on and in the bark and wood of trees at all seasons of the year, I have never seen them dig circles of holes in the bark. This, of course, is negative evidence, but negative evidence over a long series of years is valuable, especially as it is borne out by all other ornithologists with whom I have spoken. Even Mr. Forbush admitted this in his own case as quoted above. Like the Michaels, I have never found fresh rings of holes except during the time of the sapsucker migrations.

I have, however, several positive observations, recorded at the time in my notes, as regards these activities in the Yellow-bellied Sapsucker. The first of these is dated Ipswich, October 2, 1904, and Mr. Ralph Hoffmann was with me at the time when I stated that we watched "an immature sapsucker drilling a ring of holes in an apple tree. No sap apparent." This is my first positive observation.

The next record is of considerable significance in this discussion, and had I seen only the latter half of the drama, my conclusions might have been different. In the Wenham swamp on May 11, 1906, my notes state that Glover M. Allen and I found a Yellow-bellied Sapsucker drilling holes in a white pine. His movements were slow and he paid little attention to us standing below him at the foot of the tree. When he departed, a female Downy Woodpecker visited the holes. I would add here that this is the only time in the spring migrations I personally have seen a sapsucker at work in eastern Massachusetts. Mr. Wendell Taber tells me that he watched a sapsucker boring and drinking sap at a hawthorn tree in the Public Gardens in Boston in May, 1931. I visited this tree a few days later and saw that considerable damage had been done by the numerous large holes and that the sap was still flowing. The spring migration of the Yellow-bellied Sapsucker here is generally under a month in duration, while the autumn migrations extend to two months.

In the next record, made on October 1, 1916, at Ipswich, I note "Fresh orange-colored pits of sapsucker in rings around the large trunk of an old sweet apple tree; no sap exuding from the holes. Many old rings of holes made in previous years to be seen. The sapsucker which was seen near had probably eaten the cambium." Although the inference that the holes were made by a sapsucker is a probable one, it is, in absence of direct evidence, only an inference. Two years later, on November 3, 1918, I elaborate on this note as follows: "The sweet apple tree on the corner of the driveway is encircled with holes of the sapsucker on the trunk and limbs. Every fall, as now, I find fresh ones, perfectly dry. Either the birds are practicing drilling holes or are eating the inner cambium layer—they get no sap." A second positive observation was made on October 3, 1920: "Disturbed a Yellow-bellied Sapsucker at work making rings of holes in my sweet apple tree. No Downy near."

Another favorite tree for circular rings of holes is a Japanese walnut tree close to my house at Ipswich, and although I have a number of times seen a sapsucker near I have never actually caught him in the drilling act.

My last record was made at Ipswich on October 6, 1927, when in company with Dr. Tracy Storer of California, who was staying with me. We watched a sapsucker pounding on the trunk of an apple tree, and moving and drawing back its head as if eating or tasting. The bird then flew away and we found five fresh characteristic holes arranged in an irregular horizontal line, each hole about three millimeters deep. These slowly filled with sap as we examined them. We each tasted of this sap, found it slightly sweet and slightly bitter, the latter due, perhaps, to the bark. The next day I noted that the holes were dry.

As a result of all this, I have come to the conclusion that these well known and characteristic circles of holes are made by true sapsuckers and not by Downy or Hairy woodpeckers.

A few reflections may be added. Mr. Forbush, Dr. Grinnell and others have referred to the harmless character of these autumnal perforations, while the destructive effect on the trees of the sapsuckers' work when the sap is running in the spring and early summer is well known and admitted. I am inclined to think that my suggestive notes under November 3, 1918, may have much truth in them: "Either the birds are practicing drilling holes or are eating the inner cambium layer." In fact, both of these may be the correct reason, and, as shown in my note of October 6, 1927, they may also get a little sap.

Courtship display, song and gathering of nesting materials—all connected with the amatory instinct—have recrudescences in the autumn. May not such a specialized instinct as that of the sapsucker for acquiring food, a habit which is best pursued in the spring and early summer, have also an autumnal recrudescence?

Ipswich, Massachusetts; read October 22, 1931, at the meeting of the American Ornithologists' Union, Detroit.

OBSERVATIONS ON THE HEAD MARKINGS OF THE GOLDEN-CROWNED SPARROW

WITH ONE ILLUSTRATION

By JOSEPH MAILLIARD

In the work of banding Golden-crowned Sparrows (*Zonotrichia coronata*) at my Woodacre banding station in Marin County, California (see Condor, XXXI, 1929, p. 194), sufficient data have been accumulated in the last four September-to-April "seasons" to warrant the publication of the observations set forth in the present paper. In the beginning of this work the matter of determining the age of a Golden-crowned Sparrow at the date of banding proved to be puzzling. As "repeats" and "returns" came into the traps confidence in my power of discrimination in regard to differences in sex and age rapidly diminished on account of the variations in the head markings of supposedly immature birds. Available literature describing this sparrow gives the impression that the various authorities concerned with the description of the head markings of immature birds either "side-stepped" the issue or were content with a very meager portrayal of conditions. Ridgway (Birds of North and Middle America, Bull. No. 50, U. S. Nat. Mus., I, 1901, p. 334) gives about the best short description so far found, as follows: "*Immature* (*young of the first winter?*).—Similar to adult female, but without any lateral black stripe on pileum or well-defined median stripe, the whole forehead and anterior of crown yellowish olive, more or less flecked with dusky (sometimes with more or less indication of a black lateral stripe), the posterior portion of the pileum light grayish olive-brown, streaked with dusky." This description would be improved by adding, what some other authorities mention, that in place of the lateral black stripe on the pileum of adult birds there often is a very distinct, though narrow, brown stripe on the pileum of immature birds, particularly of males. However, there is too much variation in the head markings of fall and winter birds, except in cases where the bird is in what I shall call "full" (that is, similar to nuptial) plumage, to permit of writing out any really satisfactory description that would cover the ground. The prominent head markings in the nuptial plumage are, of course, the broad, bright yellow median stripe over forehead and crown, merging into light gray posteriorly, and the broad lateral black stripe bordering the yellow.

At the end of the first season, I had kept the traps going as long as any Golden-crowned Sparrows were appearing, in order to take note of the spring molt of the head feathers that this species undergoes; and practically every individual taken in late March and early April had either acquired nuptial head feathering or showed distinct signs of soon doing so. The few that were not actually in the molt had ample time to make the change before reaching their nesting ground in the far north. In the second autumn of this work, when birds banded in the previous season began to appear in the traps, I found that some of these "returns" showed head markings closely approaching those of the nuptial plumage, while the head markings of other "returns" were not distinguishable from those of individuals that I was recording in my note book as immatures.

The fact was interesting that the majority of these return birds, banded the previous fall, none of which could be less than a year old and all of which had acquired the nuptial head dress at least once, had again assumed what I had been calling immature markings. That seemed to confirm my idea that this sparrow did not attain a state of full plumage until the third summer of its life, that is, until the third post-juvenal molt. In the second season's work, to meet this development, I assumed that

individuals with *very distinct but still brown* lateral crown-stripe, and those showing some blackish effect anteriorly in this brown stripe, were birds of the previous year, and that those with indefinite brown lateral crown-stripe, or none at all, and with much dusky flecking over the crown and nape, were the real birds of the current year and I so tried to record them.

Unfortunately there turned out to be so much intergradation among the head markings of immature birds that nothing very satisfactory resulted from my efforts to work out reliable criteria for recognizing the successive stages of development, and finally it came down to recording the birds, on banding, as immatures or adults according to whether black or brown predominated in the lateral crown-stripes. What seemed certain, however, was that where there was little or no indication of a lateral crown-stripe and yet much dark longitudinal flecking over most of the head the bird was in its first post-juvenal plumage, and very probably a female, to judge by museum specimens. Birds in this class were recorded, on banding, as "Im"; where the lateral crown-stripe was definitely present, but neither heavy nor absolutely black, the underscoring was omitted; where this stripe was heavy, but only brown, the bird was recorded as a yearling.

When "returns" commenced to come into the traps in the third fall of banding, it became evident that my symbols, except the "Im" and the "Ad" (adult) were of little value, for here were birds that had been banded *two years previously* appearing with practically the same head marks as had some of the birds that I was calling immatures or yearlings. This made it evident that comparison of live birds, even when banded, would not entirely clear up matters, particularly as there was no reliable way of recognizing their sex, in case that should prove a factor in age determination. Museum specimens of the Golden-crowned Sparrow that are labeled "immature" are only of value for comparison when they have been made up by careful preparators who would notice in the course of preparation the incomplete ossification of skull that is shown by young birds up to, *but not later than*, the end of their first fall.

Among at least the smaller species of passerine birds the stage of immaturity is supposed to be concluded within the first year of their existence, but the returns of Golden-crowned Sparrows proved that the word "immature" must be applied to this species for a longer period and that many museum specimens that were taken in winter and are labeled "immature" are of doubtful value for age comparisons. They might have been in their second or even third winter. In consequence of this it became evident that nothing but examples of banded birds, with sex ascertained by dissection, would give the answer to this problem. In this matter certain casualties, at first deplored, helped to supply such specimens, and finally enough were assembled to show definite results. In this affair, cats, hawks and wires contributed largely.

To make sure that the appearance of similarity in head markings of individuals of different ages, up to two years old at least, was not a personal error in judgment, I arranged an exhibit of eight specimens on a tray, backs up and labels concealed from view. Five of these were of unbanded birds, undoubtedly birds of the year, of which the skulls were not yet completely ossified, taken and prepared by myself in October, 1931. The other three were banded females as follows: band no. A 176689, banded December 29, 1929, put up as a specimen December 27, 1930; A 120544, banded February 23, 1929, made into specimen January 11, 1931; A 110511, banded October 27, 1928, specimen January 11, 1931. This exhibit was submitted to two well known ornithologists and to several other naturalists with a fair knowledge of our local birds and the request was made that they should select by sight the older

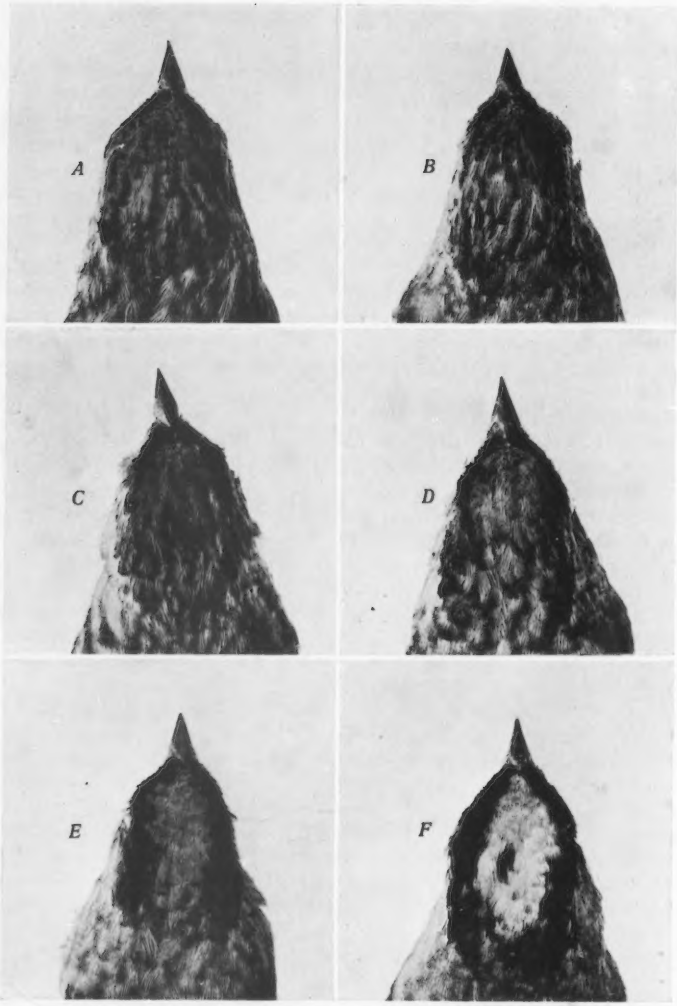

Fig. 9. HEAD MARKINGS OF GOLDEN-CROWNED SPARROWS. (A) FEMALE, BANDED A110511, OCTOBER 27, 1928, SPECIMEN JANUARY 10, 1931. (B) MALE, FIELD NO. 4021, IMMATURE IN FIRST AUTUMN PLUMAGE, SPECIMEN OCTOBER 24, 1931. (C) MALE, BANDED A120542, FEBRUARY 23, 1929, SPECIMEN JANUARY 4, 1931. (D) FEMALE, BANDED A120686, SEPTEMBER 9, 1929, SPECIMEN OCTOBER 12, 1931. (E) FEMALE, C. A. S. NO. 23753, SPECIMEN NOVEMBER 15, 1920. (F) MALE, BANDED A120776, OCTOBER 5, 1929, SPECIMEN NOVEMBER 9, 1931.

birds. One ornithologist selected two birds, side by side, as adults, but one turned out to be a male bird of the year put up by myself October 10, 1931, and the other was a female, banded in the previous fall and hence at least a yearling. The other ornithologist was confident that he could recognize the differences in age and selected one as surely being adult—but it was another bird of the year. Finally they all gave up trying to distinguish yearlings from elders. There are other banded specimens in the Academy collection that show the same similarity of markings at different ages, and my records are replete with notes proving my contention that age determination of live Golden-crowns that are not at least approaching the fully adult stage is largely a matter of guesswork.

Although the coloring of the head of the Golden-crowned Sparrow is not well adapted to photographic reproduction, the endeavor is here made to illustrate by that method the similarity of marking just mentioned. It happens, however, that differences in the "make-up" among the specimens shown made it impossible to place the heads exactly in the same position as regards the angle of reflection of light in relation to the lens of the camera, and also that some of the heads have a little more red in the brown of the feathers, which makes them appear somewhat darker in the photograph (fig. 9) than they do in reality and somewhat less similar to the others. For the illustration I am greatly indebted to Dr. G. Dallas Hanna, Paleontologist of the California Academy of Sciences, who is an expert in the photographing of difficult subjects.

Of the specimens here shown the first is a female, at least in its third year, that was not recognized by the expert ornithologists as being older than the male (B), only a few months old, that was arranged next to it in the test mentioned in this paper. Neither was the male (C) recognized as being at least two years of age, in fact nearer three. This last has more red in the brown of its head feathers and hence looks darker in the photograph, though it is in reality very similar to (A) and (B). (D) is a female, banded two years previous to being made into a specimen, which has the head in an intermediate stage between the above three and the two following that are in fully adult feathering. Of these last, (E) and (F), both November specimens, the first is an unbanded female of unknown age and the last a male banded two years previously. Both of these show head markings very similar to those of the nuptial plumage, but the crown of the female is neither quite as brightly colored as that of the male, nor are its lateral crown-stripes as wide or as deeply black.

Now, in the fourth season of banding this sparrow, sufficient data have been accumulated to warrant setting forth certain conclusions concerning the species, such as the following:

That in the spring each individual goes through a molt of the head feathers and assumes the nuptial markings consisting of a wide yellow median crown stripe, that posteriorly merges into light gray (rarely entirely replaced by gray), and a wide deep black stripe adjoining the yellow on either side. In this paper the black stripe is spoken of as the *lateral crown-stripe.*

That the male bird has generally, but not always, brighter and more strongly defined median and lateral crown-stripes than has the female, at all seasons.

That the majority of the *banded* birds that return to the traps in the fall, that is to say, birds banded in previous seasons, have the head markings of the same character as those of the nuptial plumage, but usually less pronounced, and in this paper these are called *adult head markings.*

That a large proportion of the *unbanded* birds that appear in the fall are without a well defined median crown-stripe but with the forehead more or less yellow

that merges into brown on the crown and nape, also with more or less distinct lateral crown-stripes that vary in color from light brown to blackish brown, the crown and sides of the head having an appearance of narrow flecking or streaking caused by darkening of the tip and distal median part of each feather. Individual birds vary greatly in this particular. Some have this streaking quite dark and heavy while others have but little of it, and some have a black feather or two showing anywhere in the lateral crown-stripes, even in cases where the latter are not well defined.

That some of these immature birds continuously develop black feathers in the lateral crown-stripes through the fall and winter and gradually assume adult head markings, only to shed them in the spring molt for the brighter feathers of the nuptial state.

That some of the individuals in the post-juvenal plumage can be recognized as birds of the year by the faintness of the head markings, and that these are most apt to be females, as proved by museum specimens; but that many of them, particularly among the males, have sufficiently strong head markings as not to be distinguishable from some of the birds that are of a year or two greater age, as proved by the returns of banded birds, especially of banded females.

At the present time I am recording as *positively birds of the year* only those with the faintest markings, having commenced this system in the fall of 1930. Of the birds so recorded in that season those that returned to the traps in 1931 still had only a slightly less definite appearance of immaturity than they had at the time of banding. Many birds were recorded in 1930 as only *probably* immature, and of those so recorded that returned to the traps in 1931 many appeared with strong, bright, fully adult head markings while a larger number of this class were either with duller, but fully adult, markings or showed evident sign of approaching that state.

It seems reasonable, from the facts stated in this paper, to conclude that the Golden-crowned Sparrow does not attain fully adult head markings until its third post-juvenal molt. From return birds in 1932, I hope to be able to show this even more conclusively, with the assistance of the recent care taken in age recording of the birds of the year.

California Academy of Sciences, San Francisco, December 31, 1931.

NESTING OF THE HAMMOND FLYCATCHER IN ELDORADO COUNTY, CALIFORNIA

WITH TWO PHOTOGRAPHS BY MILTON S. RAY

By ROSE CAROLYN RAY

Probably no two species of birds in California are more difficult to separate afield than the Hammond and Wright flycatchers. In our endeavor to secure the first set of eggs from California of the first-named species Mr. Ray has taken, over a long period of years, numerous flycatchers with their nests and eggs at various altitudes in the high Sierra Nevada from Lake Tahoe south to East Lake in the Kings River Cañon country. In every instance, however, until the year 1929, all birds collected proved to be *Empidonax wrightii.*

Dr. Joseph Grinnell, in his "Distributional List of the Birds of California", summed up the situation as it stood in 1915 when he stated, on page 92, "Of the many records of the breeding of this species [*E. hammondii*] along the Sierras, not one has been authenticated. . . . I am confident that all breeding records from southern California, and central California west of the Sierras, are erroneous. . . . In these and doubtless most other cases *E. griseus* or *E. wrightii* was mistaken for it. Breeding of *hammondii* in the high central and northern Sierras is not improbable, but the records to this effect need verification."

Fig. 10. NESTING SITE OF HAMMOND FLYCATCHER ON THE SIERRAN SUMMIT IN ELDORADO COUNTY, CALIFORNIA.

When, some time later, Dr. Tracy I. Storer wrote us that a breeding female of *Empidonax hammondii* had been taken in the Yosemite region on June 20, 1915, he not only proved the prediction of Dr. Grinnell to be well founded but he likewise caused us to redouble our efforts to find the nest and eggs of this elusive species. For well over a decade, however, our field work proved futile; for it was not until fourteen years later that success was at last attained.

While endeavoring on June 21, 1929, to locate a nest of that diminutive *rara avis*, the Western Ruby-crowned Kinglet, in a forest of pines and firs on the Sierran summit at an altitude of 7600 feet, I came upon a newly completed nest in a tamarack six feet up. It was placed where several small trees and a slanting sapling came together and thereby offered concealment, but it was not woven to the limb as the nests of the Wright Flycatcher are. As both in construction and location it differed from any nest that I had previously found, I later returned with Mr. Ray; but he, also, was unable definitely to determine the identity of the absent builders. On a third visit, two days later, although no birds were about, the nest contained two buff-colored eggs. As the final visit was made by Mr. Ray, alone, I take the following notes from his field book and the technical description of the nest and eggs that follows from his records in the Pacific Museum of Analytical Oology, San Francisco.

"Arrived on the summit at noon today [June 27, 1929], and I was thrilled, some time later, on reaching the puzzling nest previously found by Mrs. Ray, to see

Fig. 11. NEST OF THE HAMMOND FLYCATCHER (IN SITU) SIX FEET UP IN A TAMARACK OR LODGEPOLE PINE ON THE SIERRAN SUMMIT (AT AN ELEVATION OF 7600 FEET) IN ELDORADO COUNTY, CALIFORNIA.

the tail of a sitting bird projecting over the rim. Approaching still nearer, a small flycatcher fluttered off and disappeared in the dense forest, disclosing a beautiful set of four rich buffy eggs, one of which was lightly spotted with cinnamon-rufous and liver-brown. After a long interval the bird returned to the vicinity but remained well up in adjacent conifers, flitting from branch to branch close to the trunks of the trees and occasionally uttering a feeble *pit*. I tried a long-distance shot but missed and sent the fine shot scattering and rattling through the lofty boughs.

"As the only time the bird had come within range was when it was in the vicinity of the nest, I now, as a precautionary measure, collected the set and substituted four eggs of *E. traillii* in their place. This later proved a wise measure as one of the substituted eggs was destroyed by one of the stray, scattering shot.

"After half an hour the wary little flycatcher was again approaching, and taking a quick aim at the flitting, feathery target I fired and by a fortunate shot, the specimen was secured and the mystery solved. The bird, a male, proved to be the long-sought Hammond Flycatcher (*Empidonax hammondii*) and the first I had ever taken, or definitely identified afield, of this, at once the rarest, wildest, and most silent of all the flycatchers inhabiting the great Sierran woodland."

The nest was rather loosely made for a flycatcher and is basically composed of dark red bark strips, together with light gray bark strips, rootlets, grasses, stems, feathers, string, cocoons and woolly substances, and thickly lined with feathers. The outside measurements are, top, 3¼ x 4½ inches; depth, 2¼ inches. The nest cavity is two inches in diameter by one inch in depth. Compared with our score of nests of the Wright Flycatcher it is easily distinguishable at a glance, as the latter are all similar, being compactly woven of grayish bark strips and lined with fibres, rootlets, and some with a few feathers in addition. The four eggs, which showed but a very slight trace of incubation, have an ivory yellow ground color (Ridgway, 1912, pl. xxx). Two are unmarked; a third has a few fine specks of cinnamon-rufous, while the fourth is clearly but lightly spotted, chiefly around the major end, with cinnamon-rufous (Ridgway, pl. xiv) mixed with minute pin points of liver-brown (Ridgway, pl. xiv), in some cases even darker than the plate.

In shape they are elongage ovate, fine in texture, without gloss, and with a considerable lack of uniformity in size. They measure in inches .63 x .48, .65 x .49, .68 x .50, .70 x 56. Compared with our series of eggs of *E. wrightii* the difference does not lie in the size or shape but in the peculiar, clear, rich, yellowish tint of the ground color and the minute specks of dark liver-brown; but whether either of these characteristics would remain constant in a large series of eggs of both species is, of course, extremely doubtful.

Major Charles Bendire, in his matchless work, "Life Histories of North American Birds", vol. II, page 320, says, speaking of the eggs of the Wright Flycatcher, "eggs without luster and unspotted". With this statement, however, we disagree as we have a number of sets of eggs of the Wright Flycatcher whose surface has a decided gloss, and in one set (no. 5), taken at East Lake at an altitude of nearly 10000 feet, one of the three eggs is finely and rather generally spotted with cinnamon-rufous. Peculiarly the coloration of these spots is the same as on the eggs in our set of *E. hammondii*, excepting, of course, that the pin-point-like spots of liver-brown are absent. In collecting this set it was necessary to tramp to our base-camp and back, a total distance of sixteen miles, to get a gun to secure the parent and thus definitely establish the bird's identity.

In size we have eggs of *E. wrightii* (with which *E. hammondii* is most often confused) that are both smaller and larger than the eggs in the type set of the latter.

An egg of *E. wrightii* in set number 4 measures .62 x .51, and one in set number 8, .75 x .51. Major Bendire gives the range in size for eggs of the two species as follows: *E. wrightii*, .60 x .50, .70 x .54; *E. hammondii*, .60 x .48, .73 x .56.

Elated with my good fortune in discovering the eggs of this rare denizen of the Sierran wilds and hoping possibly to find another nest we spent many weeks during the summers of 1930 and 1931 making our way over hundreds of miles in the higher altitudes, in the vicinity of Lake Tahoe; but not a single bird was even seen.

San Francisco, California, December 21, 1931.

TECHNIQUE OF RAPTOR FOOD HABITS STUDY

WITH TWO ILLUSTRATIONS

By PAUL L. ERRINGTON

This paper has to do with the technique of some phases of raptor food habits study worked out in connection with the Wisconsin Quail Investigation, a cooperative project supported by the Sporting Arms and Ammunition Manufacturers' Institute, the United States Biological Survey, and the University of Wisconsin. Since the investigation was begun in July, 1929, I have had experience to date (November, 1931) with 73 raptor nests, namely, 21 of Horned Owl, 9 of Barred Owl, 4 of Long-eared Owl, 2 Screech Owl, 14 Red-tailed Hawk, 10 Marsh Hawk, 6 Cooper Hawk, 3 Duck Hawk, 2 Red-shouldered Hawk, 1 Broad-winged Hawk, and 1 Sparrow Hawk. In addition to the nesting studies, much field observational work on hawks and a rather large scale examination of owl pellets have been carried on. Special emphasis has been laid on determining direct and indirect relations of each raptor species to poultry and game birds, particularly quail. The food habits data as such will be published separately.

It was soon learned that hawks and owls were not equally amenable to the same methods of study. Methods invaluable for the study of one group proved of negligible utility in the study of the other. It was likewise learned that, where possible, distinction should be made between quantitative data which give a cross-section of the food habits of a raptor species and qualitative data which show only the kinds of prey taken without reference to the proportion of each kind.

Let us turn to a discussion and tentative evaluation of the various research methods employed.

Field observation. Field observations may be either accurate or misleading, according to whether reliance is placed upon actual observation of the capture or eating of prey under typical conditions or upon the independent reading of sign. A criticism to be made of sign reading, even when done by competent field men, is that large conspicuous avian prey is almost certain to be recorded out of all proportion to small mammalian prey. The feathers of a quail, for instance, are promptly spread over so much territory that they become noticeable even to casual observers, whereas the visible sign marking the demise of a mouse or shrew may be so imperceptible that little or no trace can be found, even should one scrutinize the spot where a hawk has been witnessed to catch or eat one of the smaller mammals. Similarly the counting of skeletal remains about Wisconsin Duck Hawk feeding ledges led to an impression that Peregrines subsisted on a nearly straight diet of domestic pigeon, while a checking over of contemporaneous pellets revealed a quite surprising representation of lesser bird life. The reading of sign *alone*, therefore, I feel to be conducive to a warped perspective. Its value, supplementary only, is in ascertaining whether *any* of some species such as quail and ruffed grouse have been killed. It should never be used in working out ratios of one kind of prey to another.

On the other hand, the listing of entire or partially eaten specimens *retrieved* from hawks gives a more reliable index to day-by-day food habits, though a hawk is somewhat more likely to be surprised with larger than with smaller prey, as the former requires more time to eat. Marsh Hawks bringing food to their nestlings can be excited into relinquishing what they have by approach of human intruders to the nest. They usually drop and recatch their quarry a number of times before letting it go. The falling object may then be marked down and procured.

The field observation method has been used in Wisconsin to advantage on Red-tails and Marsh Hawks. For obvious reasons it is not so easy to apply to nocturnal predators.

Nest studies. Raptor nest studies, like field observations, are possessed of potentialities for quantitative data, but must be handled with discretion. Quantitative data of unquestionable merit as to the diets of the young are obtainable by keeping nests under constant observation, as from a blind; but this method has the weakness of being so time-consuming that too few nests can be watched in a season. Data from but one or two or three nests, regardless of how complete for those nests, may or may not be representative even of the local food habits of the species studied. Every once in a while raptors of non-typical food habits are encountered, the proper allowance for which can not be made unless data are had treating with large numbers of the species.

Nest visits, conducted at intervals of a day or two, have been most profitable the first few days after hatching out of the young, when the nestlings were unable to eat all of the food brought in. The nests yielded less and less as the nestlings became large enough and hungry enough to clean up the prey, though valuable data were still to be derived from young hawks by examination of their gullet contents. Feather material persisted for varying periods of time, while only the barest traces of unknown numbers of small mammalian prey remained evident in the majority of nests. Thus the proportion of bird to small mammal prey recorded from ordinary nest visits (both for hawks and owls) tended to exceed the proportions determined by tried quantitative methods. This has been repeatedly checked. Mice, for example, occurred commonly in the pellets of nestling Horned Owls and in the gullets of young Marsh Hawks, yet seldom was corresponding evidence of mice to be seen in the nests.

Despite the general disadvantages of the method—to which should be added the personal risk and labor of heavy routine climbing, even with accessories as ropes and climbing irons—it was found desirable to examine the nest litter of Horned Owls and Red-tails about once a week. Observations indicated that feathers of quail and ruffed grouse, especially primaries and tail feathers, working deep into the interstices of stick nests, would remain there indefinitely, unless deliberately removed by hand. In this way an investigator, assigned to a particular game bird species, may be enabled to employ another check (not claimed utterly infallible) as to whether that species is brought into a given nest. Stick nests, that may be used on successive years or perhaps in the same season by Red-tails, Horned Owls, etc. (a late nesting raptor will sometimes take over a recently vacated nest of another), should be given a careful cleaning soon after the contained eggs hatch, to eliminate possibility of avian prey from a previous occupant being diagnosed as the prey of the wrong predator. For that matter, where feasible, a careful clean-up should also be made at each visit.

Stomach examinations. Drawbacks to the exclusive use of stomach data have already received space in literature. Stomach data at best show only what individual animals in a given locality have eaten at a given time. At worst they show only part of what individual animals have eaten some place, some time, especially where differentially resistant contents are in advanced stages of digestion.

It may be stated, preliminary to discussion later on in this article, that owl stomachs furnish much more reliable data as a rule than hawk stomachs, for the reason that bony material suffers less from the digestive processes of owls than from hawks. On the other hand, feathers and fur appear to survive digestion better in hawk stomachs than in those of owls. These generalizations are based upon feeding experiments with 6 Horned Owls, 1 Barred Owl, 1 Long-eared Owl, 3 Red-tailed

Hawks, 3 Cooper Hawks, 1 Red-shouldered Hawk, and 1 Marsh Hawk, as well as upon my own 1929-31 experience with 59 miscellaneous raptor stomachs and many thousands of pellets.

I have never been able to regard stomach analyses as contributing the best quantitative data, except in the case of species having such monotonous food habits that their diets would remain about constant wherever and whenever they fed.

For one thing, the number of stomachs examined representative of a given set of conditions is usually too small to have much mathematical significance. The number of raptor stomach analyses of which American scientific institutions have record only totals up to a relatively few thousand for all species, and the specimens were collected over a long period of years, taken at all seasons and from all sorts of environments.

While they may give a good general idea as to the kinds of food and the frequency with which they are taken, country-wide examinations of individual stomachs from a versatile feeder like the Horned Owl may fall far short of reflecting the local food habits of the species—which local application, I understand, is one of the primary aims of food habits research. Wholesale stomach examinations cannot be expected to remedy this defect unless maximum consideration be given to dynamic ecological factors which profoundly modify the food habits of at least some species—ordinary fluctuations of native plant and animal populations, ascendencies and declines of exotics, drastic environmental changes brought about by man, fire, flood, drought, deep snows, etc.

It is evident that a definite local program of raptor study based upon stomach examinations would demand each year perhaps hundreds of stomachs if the data were to have high quantitative rating. It is evident furthermore that such an ambitious program might necessitate a heavy killing of birds of prey to supply the requisite number of stomachs, a certain proportion from species already depleted from human persecution or other causes. Irrespective of scientific ends, undue pressure upon rare or waning species is hardly consistent with the principles of conservation, particularly when many of those species can satisfactorily be studied by less lethal methods.

However, while stomach examination as a major method of study may not be all a method ought to be—no method is—it still remains about the main source of pre- and post-nesting data on Buteos and Accipiters. The Buteos are among the most commonly shot hawks in many communities, notably in the West, and can often be seen strung up on wire fences, nailed to the sides of barns, etc. In cold weather or where dessication occurs without decay and the approximate date of killing can be established by inquiry or by the condition of the carcasses, considerable stomach data can be procured incidental to automobile trips throughout the country and at no further sacrifice to the species. Besides, the opening of a dozen or more Rough-legs and Swainson Hawks in the presence of the farmer who shot them may have decided educational value.

The elusive Cooper Hawks and Sharp-shinned Hawks are of the few of our raptors, which, in view of their apparent ability to hold their own against man and their ill repute—though it should be admitted that we know next to nothing of the true ecological role of these birds—might be collected on almost any reasonable scale at present, *by persons who know hawks*, without foreseeable detriment either to the species or to human interests.

All in all, the investigator should strive to let no hawk or owl stomachs go to avoidable waste, whether those birds were shot wantonly or for an intelligent motive. Why cannot use be made of the uncounted thousands of hawks annually slaughtered

along the highways in the southwestern United States? Indefensible as may be the practice of shooting those fine Western Red-tails and Rough-legs for little reason than because they make conspicuous targets on telephone poles, yet the victims have nothing more to lose by having their stomach contents analyzed. And what about the shops of obliging small-town taxidermists as sources of stomach material?

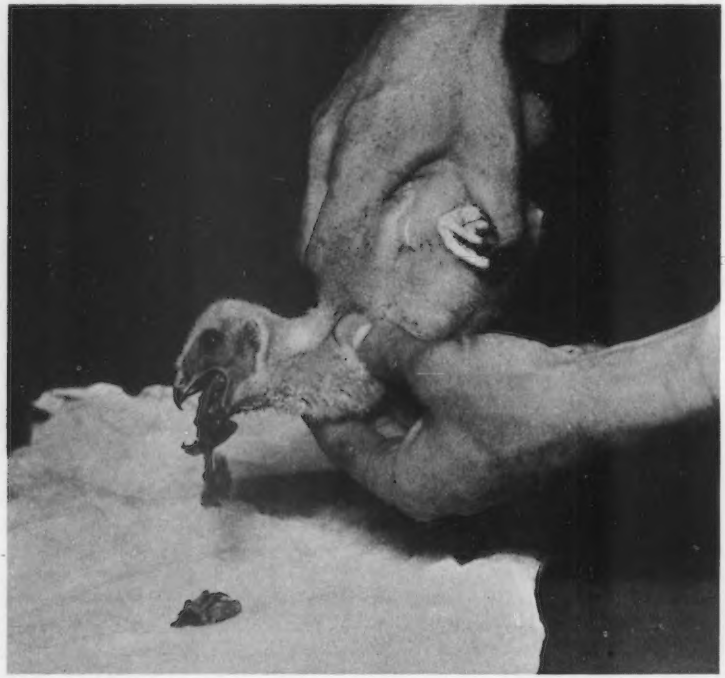

Fig. 12. JUVENILE MARSH HAWK SHOWING TECHNIQUE OF GULLET SQUEEZING.

Gullet examinations. Comparable to stomach examination, but lacking some of the disadvantages, is the gullet examination method of studying the food habits of nestling hawks. Food swallowed by all hawks studied (not owls) did not go directly to the stomach but was retained in the enlargement of the esophagus, functional as the crop, the period of retention varying according to the age and rate of development of the nestling. It was observed in experimental raising of young raptors, that Cooper Hawks one to two weeks old, when well "stuffed" at 8:00 a. m. still had a reserve of food in their crops at mid-afternoon, whereas older juveniles slipped, bit by bit, their gullet contents into their stomachs at a more rapid rate. The same seemed true of Red-tails and Marsh Hawks.

Now this gullet food material can be forced upward and out of the mouths of nestling hawks, day after day, without injury to the birds. (See figs. 12 and 13.) In the 1931 study of Marsh Hawks, the youngsters were robbed of their meals once and twice a day for the duration of the nesting season and for some time afterward.

The young were tethered near the nests when they became well feathered and ready to fly; thus they remained available for study a total of about two months, as the old birds continued feeding them up to a month or so past the date that the young normally would have left.

The quality of the food habits data obtainable by squeezing the gullets of young hawks is considered of the highest excellence. The material is fresh, with meat, bones, skin, fur, and feathers unsubjected to gastric action. The material is representative of the food that the adults regularly bring the young and is fed in sufficiently large pieces to give an analyst something to work with. If the gullet contents, taken together from all the young in a nest, be sorted out and wings, feet, tails, etc., matched up, what appear to be accurate quantitative data may be secured. In all instances where the food of both adults and their young could be checked up on (this was done with no hawks, but with Horned and Barred owls), no major discrepancies in diet were discovered, though it is suspected that the adults eat more of the smaller prey themselves.

The suggestion might be made that young hawks relieved of their meals two or more times a day be force-fed at the last visit in late afternoon or evening, so as not to deprive them completely of their nourishment. Non-diagnostic portions of prey may be fed, or the investigator may carry with him fragments of wild ground squirrels or cottontails (with plenty of bone and liver to furnish the calcium and vitamins essential to healthy growth). Where supplementary feeding is necessary to make up for systematic robbing of the youngsters, precautions should be taken not to feed within eight hours previous to the next visit, to allow ample opportunity for the gullets to become empty of items having no place among the food habits data. The most likely time to find food in the gullets of Marsh Hawks and Cooper Hawks was about an hour before dark or about eight or nine o'clock in the morning, although one could never be assured of reward for all visits after the young attained growth.

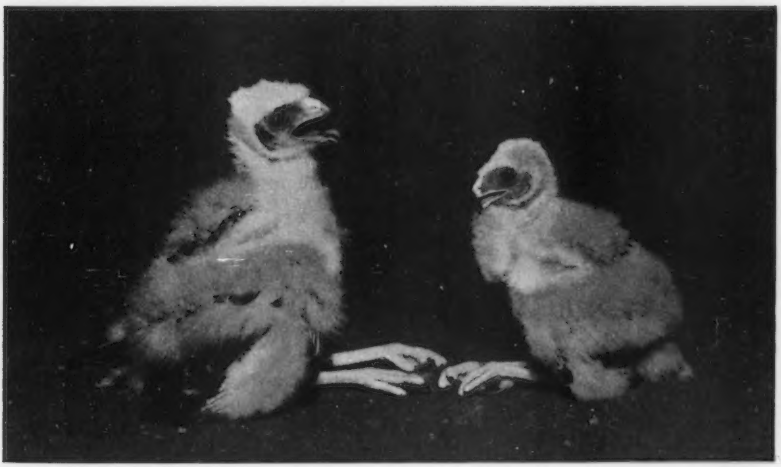

Fig. 13. JUVENILE MARSH HAWKS. BIRD ON LEFT HAS GULLET EMPTY, ONE ON RIGHT HAS GULLET FULL.

Pellet examinations. Preliminary experiments in 1930 (Errington, Paul L. The Pellet Analysis Method of Raptor Food Habits Study. Condor, XXXII, 1930, pp. 292-296), in which captive raptors were fed various kinds of prey to determine the analytical value of pellets, showed broadly that pellets reflected quite consistently the food habits of owls, while the pellets of hawks were not so satisfactory. The reason for the comparative inadequacy of hawk pellets has already been indicated: the skeletons of prey did not hold up well under hawk digestive processes. Digestion of bones was especially pronounced in the Buteos, in fast-growing young hawks having high calcium requirements, and in most cases where soft-boned juvenile prey was eaten. Marsh Hawk and Peregrine pellets, though usually inferior to those of owls so far as bones were concerned, may be regarded as the best hawk pellets worked with.

The second summer's experiments (1931) substantiated on the whole the earlier results and were productive of some additional data. Perhaps the most significant experiments were those performed with grown juvenile Horned Owls, by which it was demonstrated that characteristic bones (tarso-metatarsi) of extremely immature quail chicks—chicks as small as 13 grams or about 10 days old—withstood digestion well enough to be recognized in pellets. The tarso-metatarsi of quail chicks 15 to 20 grams in weight could be made out in the pellet debris without difficulty. Birds one-third grown (70 grams) were easily distinguishable in pellets of mixed composition by the larger long bones and mouth parts. If game as delicate as tiny quail chicks leaves recognizable trace, we can surely expect that few items in the diet of adult Horned Owls (and likely the other Wisconsin owls) are apt to escape representation in the pellets. The calcium demands of very young owls, however, were so great that bony material rarely came through, a recently hatched Horned Owlet ejecting at the start boneless pellets on practically any rough diet.

The pellet analysis method is especially suited to the study of owls on account of the ease with which first-rate pellets can often be found. Wild Horned Owls under observation the past two years displayed inclinations to roost in certain trees—usually in the vicinity of old red-tail nests which were eventually preempted—under which roost trees their pellets accumulated. The trees favored were those to which leaves clung during the winter, those the tops of which were entangled with vines, those with broken, hanging tops, or those otherwise promising sanctuary to owls not desirous of spending the daylight hours in the entertainment of crows. Horned Owls were known to alternate between perhaps a dozen trees in an area of a few acres from fall until vegetation leaved out in the spring; when the foliage became uniformly dense they revealed no partiality for any tree unless situated in some strategic relation to the nest.

Long-eared Owls exhibited roost preferences as did Horned Owls. Their roosts were to be found mainly in evergreen clumps or in the willow and alder growths along creeks. In some retreats hundreds of pellets would be in sight at once. Short-eared Owls often perched on particular fence posts, corn shocks, or hay cocks about which their pellets could be picked up. This statement refers only to their winter behavior, for no personal observations on the summer habits of these owls were made. Saw-whets preferred to headquarter (winter) in red cedar patches. Winter data were obtained on four Barn Owls, three of which habitually frequented crevices in an old rock quarry, and the fourth stationed itself in a planting of white cedars. Screech Owls likewise appreciated conifers in winter, retiring also to convenient holes in old stubs. The same seemed generally true of Barred Owls. These latter two—Screech Owl and Barred Owl—have proved the most baffling of resident southern Wisconsin owls to study, for the reason, I think, that they leave pellets in their holes. What-

ever the explanation, I have simply been unable to locate their pellets in desirable quantities.

Is there danger of confusing the pellets of one owl with those of another species? A Horned Owl pellet may range in size from a ball ¾ inch in diameter, containing the remains of a single wood mouse, to a club-like mass of bone and fur 6½ inches long and 1¾ inches across. Barred Owl pellets made up of mice may be indistinguishable from Horned Owl and Barn Owl pellets made up of the same prey, and some Barred Owl pellets may resemble those of the Long-eared Owl. It is scarcely conceivable that anyone could tell the difference between Long-eared and Short-eared owl pellets merely by looking at them. Going down the scale, Long-ear pellets grade into Screech Owl, and Screech Owl into Saw-whet. Yes, there *could* be danger, but—

The Wisconsin studies have failed to bring out an instance of one species of owl inhabiting the same roosting territory with another species for any length of time. Aside from wide differences in species habitats, it appears to be the rule that when a small owl ventures into the territory of a large owl, the small owl either ventures out again or ceases functional existence as the original owl. Hence, when a Horned Owl or a Long-eared Owl or a Saw-whet Owl is seen now and then to occupy a given roost, the chances are trivial that the fresh pellets under that roost or adjacent roosts are deposited by any except the regular occupant. And granted that occasional pellets, considered singly and without accompanying data, may be mistakenly identified, pellets in *lots*, from beneath *known* roosts have run more or less typical.

Where identification of an isolated owl pellet is for some reason deemed of special interest, and where the pellet itself does not betray its identity (Horned Owls, for example, feeding on fairly large prey, break heavy bones in a way not possible for less powerful predators), a thorough familiarity with the Strigine fauna of the locality is the greatest—not absolute—safeguard against error. The weighing of probabilities, the finding of a feather or a track in the snow, the following up of some obscure lead, the "playing of hunches," altogether may help one orient what evidence he has until it begins to mean something, even if the evidence from "sign reading" in itself may be glaringly inconclusive. The thought will bear repetition that the foregoing indirect means of tracing the origin of a pellet are suggested for use only under extraordinary circumstances; ordinarily one should reject outright all questionable pellets.

Prospective investigators might be cautioned against gathering up, supposedly as one pellet, fragments of two or more, for such could be ruinous to quantitative data. Where several pellets have been broken and intermixed, as beneath a high Horned Owl roost, some pellets may be pieced together on the basis of size, age, fitting of broken ends, or distinctive coloration. Where the task of putting pellets together again with accuracy is hopeless, all of the pellet material may be gathered up in bulk, and the bones separated out to give the tally of individuals of the prey.

A yet unfathomed weakness in quantitative methods has to do with the correct allowance to be made for the representation of one medium-sized or large kill in more than one pellet. How many meals does a rabbit last a Horned Owl? If domestic chicken remains comprise three Horned Owl pellets, have three chickens been killed, or two, or only one?

Further experiments in 1931, and field work with Marsh Hawks, Cooper Hawks and Red-tails modified little the 1930 conclusions that any prey having distinctive fur, feathers, or scales would probably be represented in hawk pellets and in a condition recognizable by a skilled analyst, regardless of how badly decomposed the bones.

In general it might be recommended—with reservations—that no pellet material save bones be accepted for quantitative studies. I see no way to tell whether a given amount of meadow mouse fur in a boneless pellet represents the fur of one mouse swallowed whole or the accumulated fur of several partly skinned mice. The same principle applies to other species of prey common enough to occur twice or more in one pellet. In addition there is reason to believe that young Red-tails and Marsh Hawks pick up and swallow odd feathers detached from prey eaten at the nest or feeding place days before. A captive Marsh Hawk had exactly this habit, which, if not but the vagary of a captive, may explain the occurrence of meadowlark feathers in an extremely high percentage of juvenile Marsh Hawk pellets, while contemporaneous field observations and gullet examinations showed that no more than 3 per cent of the vertebrates brought in were meadowlarks. Determinations based solely upon bones should obviate the likelihood of such feather contaminations receiving disproportionate emphasis.

Theoretically, though the undigested bones in a given lot of hawk pellets may not represent a fifth or a tenth or a twentieth of the total of animals eaten, since the animals last eaten before the ejecting of the pellets suffer the least digestion, the bones should, according to the laws of chance, come through roughly in the proper prey-to-prey ratio. Actually, allowances must be made when the prey is exceptionally large or exceptionally small (or immature); in the former case bones are not always eaten; in the latter, the bones may be so delicate that they may not last through a rather brief period of digestion. It seems plain, then, that even bone data from pellets of some hawk species may have but meagre, if any, quantitative status.

Pellets of Buteos gave fair qualitative data but poor quantitative. They were difficult to find save in nests, and those from nestlings were the worst to do anything with. Pellets of nestling and juvenile Cooper Hawks seemed all right for qualitative data. I have had no personal experience with pellets from mature Accipiters. Pellets from nestling and juvenile Marsh Hawks are of aid in qualitative work, not so good for quantitative. Spring and fall Marsh Hawk pellets examined (no winter pellets were had) have been richer in bone remains than those of the summer and consequently of greater quantitative value. Of all hawk pellets dealt with, those of the falcons, notably the Peregrine, were adjudged (no experimental work done) the most promising from both qualitative and quantitative standpoints, the pellets at times appearing almost owl-like as to preservation of bony contents. The superiority of Peregrine pellets I am inclined to believe due to more than one ejection a day, possibly attributable in turn to the metabolic demands of an active and skillful hunter.

The technique of tethering. A long-felt weakness in the Wisconsin raptor studies up to 1931 has been the utter lack of certain species data for certain months of the year. The quantitative data pertaining to hawks have been confined largely to the nesting season; pertaining to owls, nesting season plus winter. The nesting data of necessity terminated when the young left the vicinity of the nest. It was apparent that if the young could be prevented from leaving, the period of productive study might be lengthened. This was accomplished by tethering youngsters on the ground where they could be located and fed by the adult birds. By this method a number of gaps in the seasonal food data of some species were filled in.

The tethering method was tried on Horned and Barred owls, Marsh Hawks, Red-tails, and Cooper Hawks. With Horned and Barred owls it worked splendidly, the young of these species still being faithfully cared for by the parents until released August 8. Parent Red-tails and Marsh Hawks were not as devoted as the owls but attended their offspring from a month to six weeks past the time that the latter ordi-

narily left the nests. Attempts to tether out Cooper Hawks were discouraging, as two out of three young broke their soft, spindly legs in the most comfortable of leather anklets, and the third was soon deserted by the mother. Accipiters are so high-strung anyway that one studying them can never seem to predict exactly what is going to happen.

The apparatus for tethering a hawk or owl consisted of a non-slipping leather anklet wired to a swivel which was in turn fastened to a 12- to 18-inch length of chain. The end of the chain was stapled to the base of a tree for woods raptors, or wired to an iron stake for use with Marsh Hawks. Where pellet studies were depended upon (owls), the youngsters were tethered on sloping ground so that the pellets rolled out of reach of the captives and were not trampled into worthlessness. A slope of around 30° was satisfactory if the site had been cleared of brush, leaves, sticks, etc. Visits to tethered owls at intervals of a week to ten days sufficed unless carrion beetles were active, in which event it became necessary to gather up the pellets two or three times a week. Tethered hawks were visited once or twice a day for gullet contents as long as the visits continued to yield worth-while data. It was also possible after gullet squeezing had ceased to be profitable to check upon hawk food habits in a crude way by examining the pellets of the tethered birds.

There are important precautions that should be taken in the tethering of young birds to insure a minimum of misfortune. All anklets should be of pliable but stout leather, one-half to one inch in width, according to the species to be held, and should be sufficiently loose-fitting that no injury be done to the captive's foot. A little superficial chafing above the toes is nothing to cause concern, if circulation is not impaired.

For small raptor species heavy tether chains are not needed, but it should not be overlooked that chains like those on small steel traps may be unequal to the wear and strain put upon them by Horned Owls. Suitable lengths for tethers can be cut from side sections of worn-out tire chains. Medium weight copper wire has been found adequate for the binding of anklet to swivel and swivel to chain, and likewise for securing the anklet about the tarso-metatarsus of the bird.

The most opportune time to tie up juvenile arboreal raptors is shortly before they are able to launch forth in a true flight, or in other words, when they flap to the ground as an intruder climbs to the nest. Young Marsh Hawks should be caught and tied before they become difficult to locate on account of their sneaking off and hiding in the vegetation.

Although the investigator must reconcile himself to some mortality among his tethered raptors, he can take measures to keep the losses down. If the ground beneath the nest is too much in view of passers-by or over-run by dogs and stock, better places for tethering can sometimes be found 50 to 100 yards distant. Young hawks and owls are able to acquaint their parents as to their whereabouts, within limits. Biological Survey bands spared two tethered Wisconsin owls immediate disaster at the hands of human discoverers, who, while they didn't know what everything was all about, attributed purpose and authority to the bands. Possibly explanatory placards, situated to be seen only if the bird were seen, could be used. Marsh Hawks should be staked where they can draw back on hot days into whatever shade is available. Finally, unless there is good reason why they should not be, the youngsters should be released as soon as it becomes noticeable that the old birds are growing neglectful.

A warning might be interpolated here relative to dealing with powerful young raptors, for the pedal equipment of a grown juvenile Red-tail or Horned Owl can do an incredible amount of damage in an incredibly short time. One should not court injury from parent raptors displaying frenzied anxiety when the sanctity of

the nest is violated. The ripping talons of mother Horned Owls are most to be guarded against, but incessantly swooping Marsh Hawks may enliven matters also. No personal difficulty was had with other species.

In all cases where the adults were unusually hostile (there is much individual variation as to temperament), actual attacks were averted by the *mere presence* of a second person. A lone investigator can ward off attacks by a wave of the hand or a flourish of a stick, but he is often in an unhandy position to do this. If one is visiting Marsh Hawk nests alone, he can lie on his back as he chokes up the gullet contents of the nestlings, keeping boot or shoe above him in the air for the parents to strike. Marsh Hawks are too weak to do much slicing anyway. But if one is alone and wants to climb up to a Horned Owl nest, and the old bird sits in the next tree, snapping bill and "talking"—the answer is *don't*! Twice under such circumstances have I been lacerated by Horned Owls.

Interpretation of kills. Of fundamental importance to those carrying on a life history study of a given species is the ability to read sign when a kill of that species is found. This ability is acquired slowly and never can begin to approach perfection, if for no reason other than the imperfection of the sign itself. Always must the investigator watch himself, *lest he find out things that aren't true.*

Perhaps it would be in order to review a specific incident. On December 24, 1930, the remains of a quail were seen in a railroad ditch south of Madison, Wisconsin. The sign was two days old. An empty shot-gun shell lay on the grade 60 feet from the quail and there were two wads on the snow between quail and shell. No shot pattern could be located. Cat tracks were noted about the quail remains, and it was obvious that the cat had partaken thereof. A search in the snow brought up a portion of a quail skull, with marks of two canine teeth where a piece had been bitten off. The tooth marks were too close together for cat. To the side of the track-beaten space were rather indistinct prints of mink. The canines of a mink skull checked with the marks on the quail skull. But along with the quail skull a wing had been dug out of the snow—a wing showing the typical clean plucking of an accipitrine hawk. Since a wintering Cooper Hawk had been flushed on December 20 from a warm quail killed out of a neighboring covey, the evidence was now about as clear as circumstantial evidence ever is.

This kill, fairly fresh and rendered conspicuous by snow, is illustrative of how in one way or another the fleeting records of the wild fade into illegibility. Suppose that the wing had disappeared, or both wing and skull? Suppose that the kill had not been seen until days later, and the site had been inspected in the meantime by weasel, fox, Red-tailed Hawk and other flesh-eaters not averse to scavenging when that is easier than hunting? Suppose that the kill had taken place in summer, a week previous to discovery, and rains, putrefaction, maggots, ants, and what not, had had their part in the obliteration of the story?

I do not know what are the possibilities for accurate *ex post facto* kill determinations. So far as I am aware, it appears to be a nearly universal trait of raptors (except falcons) to pluck the primaries from avian prey of the sizes convenient for handling, though this doesn't always hold true for larger sizes. Occasionally the entire outlines of raptor beaks were to be seen on primaries of plucked prey, which impressions, if of characteristic size or shape, served well in identification. In the majority of instances, unfortunately—and this has been checked experimentally—there did not seem to be imprints that one would be justified in accepting as wholly diagnostic. There is, too, a dangerous liability of a person seeing amid the confusing array of marks present on almost any set of primaries, evidence of about whatever

raptor species he might have in mind. Bill marks of the larger raptors have been the ones most likely to be recognized. On the whole, while I have not been blessed with any spectacular success from the scrutiny of hawk- or owl-plucked feathers, I am convinced that this or some comparable method, reduced to a technique finer than my own, may prove our "best bet" for the solving of old kills.

Of course, the various leads—site of killing and picking, probabilities based on intimate knowledge of local raptor species, habitual and adaptive behavior of the species preyed upon, and sundry minor clues—all may be of assistance, but, in the absence of anything indubitably characteristic, one cannot conclude a great deal with certainty. One should never be hasty in the interpretation of what is to be seen; it is too easy to record opinion as fact. By designating in his notes the data and the specimens that are doubtful, one can spare himself much grief.

Work from predator to game, that is, pellet analyses and other quantitative methods, in connection with the Wisconsin studies, frequently contributed the final check upon kills diagnosed in working from game to predator (sign reading, intensive and prolonged observation of quail coveys, etc.). Of six Ruffed Grouse kills by Horned Owls, pellets corresponding to four as to age and proximity were found. A number of times quail vanished from observational coveys, to be later retrieved from Horned Owl pellets or from nests. How could it be known whether a quail, the remains of which had been found in a Horned Owl pellet, had been taken from one covey or from another, assuming that the quail had not been banded? Familiarity with the current food habits of the surrounding quail population was of great aid in tracing victims to their coveys, on the basis of undigested quail food contained in the pellets.

Experimental technique. The Wisconsin studies with captive raptors, devised to check experimentally certain phases of normal food habits (pellet formation, handling of prey), were beset by trouble from three principal sources: (1) the physical and (2) the psychic condition of the raptor, and (3) the food. Apart from experimentation with very young birds, to which no allusion in this section is intended, *hungry, half-wild, experienced killers* given access to *live prey* were productive of incalculably superior results.

Half-wild birds were most likely to have an optimum balance of independence, resourcefulness, and tolerance toward confinement, and hence most likely to behave naturally. Very wild raptors, especially sensitive hawks bewildered by captivity, were not reliable, nor were always those raised as pets. A very wild Red-shoulder had to be force-fed at each feeding, the effect of which on its gastric economy made an intriguing topic for conjecture. A very tame Red-tail, acting as though obligated to gulp down food given it at any time, was known to eat before ridding itself of its customary morning pellet, thereby causing over-long retention of that pellet, once for several days.

The difference in results arising from the feeding of living and dead prey to the same raptor may be so pronounced as to render valueless some of the conclusions derived from experiments in which this point was not taken into account. A raptor's clutch on dying prey elicits on the part of the raptor a series of reflexes not confined to the killing; responses manifested by jerking out the primaries, tugging at the head, and "mouthing," all more or less bound up with the possession of something alive or very recently alive. Horned Owls and Red-tails given live rats, ground squirrels, quail, or young chickens regularly ate the heads first; if offered the same prey dead and cold they often started eating almost any place on the carcass. Cooper Hawks

ate all of the heads except the mouth parts of sparrows which they killed, but discarded whole the heads of sparrows tossed to them dead. Much irregularity in the plucking of primaries of dead birds was also noted.

Why this paper? I labor under no delusions that my methods are entirely new; they most obviously do not represent the last word in raptor food habits technique. I am submitting them for the reason that they have served best of any the purposes of the Wisconsin investigation, and in the hope that they may be of utility to kindred wild life research projects elsewhere. By means of these methods it has been possible to gather large amounts of presumably sound data, despite the fact that the time spent upon birds of prey has been strictly limited by other duties. The food habits of the Horned Owl have been followed through nearly all months of the year, and the methods show equal promise for some other owls. To the study of hawks they have not been so well adapted, but have helped to patch up some of the as yet lamentably ragged seasonal data.

And these are methods which conscientious persons having no special training in analytical work but having economic or esthetic interests in wild life may perhaps find practicable. One engaged in game management, for example, could hardly aspire to the technique of those who make stomach and pellet examination their profession, nor could he be expected to build up a reference collection of bones, feathers, etc., completely adequate for all possible needs. Nevertheless, he could by earnest application familiarize himself with significant anatomical details of common small mammals, game birds, poultry, and other forms of consequence. However unpolished his findings, his specimens, if properly labeled (raptor species, date, and locality, in lead pencil or India ink) and preserved (dry pellets in glassine bags, perishable material in alcohol or 5% formaldehyde), could later be looked over by qualified analysts.

In particular, pellet and gullet examinations for owls and hawks, respectively, with the period of profitable study extended by the tethering of young birds, are productive of the quantitative data imperative to an unbiased insight into the ecology of creatures that eat and are eaten. The ecological viewpoint cannot be over-emphasized if our data are to mean the most. We should not stop with quantitative data any more than we should be satisfied with qualitative. To achieve something of an understanding of raptorial relationships, we must delve deeper.

Does a given predator species take a given type of prey because of preference, because of convenience, or because of inability to take other types? What is inherent in predator species to enhance or to delimit their ability to capture prey: intelligence, patience, persistence, special senses, strength, stamina, speed, agility, facility of movement in the environment of the prey, racial modes of hunting? What are the factors governing the ability of the prey to escape, aside from inherent endowments matching those of predacious enemies? It is not enough to record that on such a date and in such a locality some raptor ate quail. Was the quail mature and in good shape, starved, crippled, diseased, carrion? What was the role of weather, accident, human agencies? The role of food-cover combinations, comparative population densities of quail, predator, and "buffer" species?

That our present research mechanism has not answered—or cannot answer—these questions as well as we might desire, does not preclude any possibility of that mechanism being expanded to do so. If it is not feasible to expand the old mechanism, a new one might conceivably be evolved, not necessarily to displace the old one but to go on from where the old one left off. Ultimately we must look forward to going on.

Madison, Wisconsin, December 7, 1931.

THE TWO CHECK-LISTS OF 1931—A CRITICAL COMMENTARY

By JOSEPH GRINNELL

The year 1931 saw the publication of the Fourth Edition of the American Ornithologists' Union Check-list of North American Birds, brought to completion and edited by Dr. Witmer Stone, chairman of the Union's Committee on Classification and Nomenclature, and also the publication of volume I of Mr. James Lee Peters' much greater undertaking, a Check-list of the Birds of the World. The former book represents "finished business", at least for the time being; the latter is but one part in a series which will not be complete short of at least ten volumes and which will absorb all the time of one man for a minimum of fifteen years. The appearance of these two books of the same general nature and significance in a single year is epochal as regards America and would seem to justify more extended analysis and critical comment than is generally given in book reviews.

Of course it is the Check-list of North American Birds[1], the volume complete in itself, that is of greatest and most immediate interest to the majority of *Condor* readers, and I will devote most attention to it. This, in its three preceding editions, has been the standard for bird names in America for 45 years. In the first edition (1886) a total of 951 forms (species and subspecies) were listed as in good standing; in the second edition (1895) there were 1068 forms; in the third edition (1910) there were 1196; and in the present, fourth edition (1931) 1420 species and subspecies of modern birds are regularly listed. The first three editions were prepared by committees under the chairmanship of Dr. Elliott Coues and of Dr. J. A. Allen, successively.

The fourth edition of the A. O. U. Check-list serves also as one unit in a world-wide series of regional lists projected some few years ago under the general term Systema Avium. This scheme for the publication of authoritative lists of birds for each of the main zoogeographical regions originated in the British Ornithologists' Union; and it has already resulted in the issue of W. L. Sclater's "Systema Avium Æthiopicarum" (1924-1930) and G. M. Mathews' "Systema Avium Australasianarum" (1927). Our North American portion of the "Systema", it may be remarked, shows much fuller statements of geographic range than either of the others do, and it is at least quite their equal as to nomenclatural correctness and completeness of citations.

The features of the fourth edition of the A. O. U. Check-list may perhaps best be discussed in comparison with those of the third edition, with which latter all or most of my present readers are familiar. The geographic scope remains the same, that is, North America north of the Mexican boundary, plus the peninsula of Lower California. The time-limit for inclusion of new matter extended to the end of 1930, though it is to be observed that a few 1931 proposals have been entered—even a few last-minute changes which insofar as the present reviewer is aware had never been made known in print elsewhere. Established "introductions", such as Rock Dove and Starling, are now included in the main list. There are eleven of these non-native species.

The phylogenetic sequence observed in the new edition is entirely renovated—modernized as regards arrangement of both genera and the higher groups. Not only this, but within genera the species and subspecies are arranged, not in historical order (by accident of precedence in description), but in logically geographic order, from

[1] Check-list | of | North American Birds | prepared by a Committee | of the | American Ornithologists' Union | Fourth Edition | Constituting the "Systema Avium" for North America | North of Mexico | Zoological Nomenclature is a means, not an end, of Zoological Science | Published by the | American Ornithologists' Union | Lancaster, Pa. | 1931 [our copy received October 7, 1931]; 8vo (150 x 231 mm.), pp. xx+526.

east to west and north to south. The nomenclature has been sifted out in accordance with the rules of the International Code, but with deference to the rules of the old A. O. U. Code where not in conflict. One special ruling, not called for by either code, is in my opinion exceedingly unfortunate. This is the employment of but one "*i*" in the genitive singular of personal specific and subspecific names even where spelled originally with two "*i*'s"; thus, the Check-list gives for the Baird Sparrow, *Ammodramus bairdi* whereas the original describer, Audubon, christened the bird *bairdii*. A curious reversal appears in such names as *mauri*, *turati* and *craveri*, originally so spelled, but which in the Check-list appear with two "*i*'s"—this action on the ground that in each case the man's name originally ended with an "*i*" and another "*i*" must be added to make it possessive. And yet we find no such "improvement" of the specific name *lewis*, of the Lewis Woodpecker. Another case of departure from the original author's spelling is in the case of the California Blue Grosbeak, "*salicaria*" instead of *salicarius*, as the name was, and should continue to be, spelt. The safe rule, one which will surely be followed explicitly by most systematic students, is to preserve scientific names precisely as their authors originally spelled them; any tinkering whatever opens the way to everlasting changing of names on the basis of varying personal preference. The placing of accents on syllables of scientific names can almost be criticised under the same head; they are additions to the names as originally spelt—not desirable, it seems to me, therefore, even though grantedly of some use to an amateur who is trying to pronounce them.

A new feature is the use of footnotes, for the most part references to casual occurences of species or to forms which have been proposed but which have not been admitted to full standing. Such references are often useful to have easily at hand, but there is lack of uniformity in their employment; very many citations are omitted which would have served quite as justifiably as those that are given. Perhaps uniformity would have meant much more space than could have been spared for this feature—in which case probably it would have been better not to attempt footnote explanations at all, especially in view of the extensive "Summary of Changes" in nomenclature which is given (pp. 380-400).

On the other hand, great improvement is shown over the third edition, in the matter of citations of original descriptions of systematic units up to the genus. These were entirely rewritten and extended and errors in the old Check-lists corrected. Furthermore, full statements of type locality are now given, both as to original wording of designation and as now to be understood. Original bases of Linnaean names (non-binomial authors like Catesby and Latham) are now given accurately. All this, I have reason to know, is the result of Dr. Stone's laborious personal research apart from his general work in behalf of his Committee.

With respect to the progressive subdivision of genera, a trend observable in some quarters, we find the new Check-list gratifyingly conservative. For example, the Sparrow Hawks, Pigeon Hawks and Duck Hawks all continue under *Falco*; the Rough-legs are included under *Buteo*; most of the Shearwaters, recently assorted under several "generic" names, remain under *Puffinus*; all the Vireos come under *Vireo*; etc. But *Melospiza* is not merged with *Passerella*, as someone recently proposed with very good ground. Subgeneric headings are entered where appropriate, and this is, to my mind, a wholly wise course for indicating groupings of species within genera.

In just one case that I have detected has this conservatism toward the recognition of genera seemingly gone to an unjustifiable extreme. Our White-headed Woodpecker, since 1858 accorded separate generic position, under *Xenopicus*, is suddenly placed under *Dryobates*, along with Downy, Hairy and Ladder-backed woodpeckers!

And to the present reviewer this came as an absolute surprise, since he had not seen a mention of such reallocation in any current literature. Possibly some paper *has* come out, covering the morphology of *Xenopicus* and coming to conclusions showing nearest, and close, affinity with *Dryobates*; but *if* no such study *has* appeared, then sharp criticism is here due on the principle that the A. O. U. Check-list is no place for publishing innovations. A proposal of such importance should be made to the ornithological world, with full evidence, through some well-known series, like the *Ibis*, or the *Auk*, or *Journ. für Ornith.*, and then time given for counter-evidence to be sought and presented—*before* authoritative acceptance. To anyone who knows the White-headed Woodpecker in life, when, only, association of function *and* structure is manifest, the generic grouping of this species with *Dryobates* is most astounding!

A step backward in the present edition, to my notion, in comparison with the third edition, is the entry of subspecies and full species in exactly the same typography, instead of the full species being in larger type, and instead of each group of subspecies being subordinated under a single binomial heading. There results an appearance of exaltation of the subspecies, as if in scientific classification the latter are of the same rank as species, which, at least on an average, they are not. Subgeneric, generic, family and ordinal names are properly given in distinctive type sizes—and so should subspecies. Subspecies are so numerous in the fourth edition (609 of them) that their muptiplicity without any difference of printing, on many of the pages obscures the general phylogenetic significance of the Check-list. Only a person who has tried to teach ornithology can appreciate the amount of confusion created in the mind of even the adult beginner by a profusion of racial names—where the identity of the species (in the Linnaean sense) is buried in a profusion of subspecies the characters of which are not readily, or at all, appreciable to the non-systematist. This difficulty, I believe, extends far and wide among ornithologists who do not happen to be specialists in subspeciation; and it could be easily avoided, and at the same time the technical value of the Check-list preserved, by the use of differentiating typography as in the third edition. The system adopted in the fourth edition results, also, in many such unfortunate cases as the entry for the Green Jay, *Xanthoura luxuosa glaucescens* Ridgway, where there is no indication whatsoever of the authority or citation or type locality for the binomially named SPECIES, *Xanthoura luxuosa*! [The earliest name in this series of subspecies happens to apply to an extralimital race.]

Now with regard to vernacular names, the fourth edition shows here and there instances of decided improvement over usages in preceding editions. For example, we now find Eastern Mockingbird and Western Mockingbird instead of just Mockingbird and Western Mockingbird; Siberian Hawk Owl and American Hawk Owl instead of European Hawk Owl and Hawk Owl; Eastern Golden-crowned Kinglet and Western Golden-crowned Kinglet instead of merely Golden-crowned Kinglet in the first instance. These are correctly formed vernaculars, for they appreciably convey ideas concerning geographical occurrence of related forms, concerning relationships, or concerning features of distinguishment.

On the other hand, I regret to be compelled to point out, if I am to be thoroughly open in this review, that very many of the vernaculars in the new Check-list do not meet the needs of the lay student. Either vernaculars should be omitted altogether, as a few professionals now do in practice, or they should be supplied with proper regard for their easy use by the amateur or "popular" bird student whose number is hugely the greater over the dwindling coterie of us old-timers who do not like to see names changed from what they were in our own student days. In the new Check-list there remain a great many "common" names that give little or no clue to their

application such as would help the student, advanced as well as beginning, to remember them. Indeed, there are quite a few newly coined names of this character—vernaculars that, to my knowledge, have never appeared in the literature before; for example: "Dotted Wren" for our long known Dotted Cañon Wren; "Gambel's Wren-Tit" for the Intermediate Wren-tit; "Nicasio Chickadee" for the Marin Chestnut-backed Chickadee; "Barlow's Chickadee" for the Santa Cruz Chestnut-backed Chickadee; "Grinnell's Chickadee" for the Northern Mountain Chickadee; "San Lucas Sparrow" for San Benito Marsh Sparrow; "Bryant's Sparrow" for the Bryant Marsh [or Savannah] Sparrow; etc., etc.

And this brings me to another criticism—of the inordinate use of *personal* names. They are as a rule meaningless to the popular student of birds, and they tend to bring ridicule on the science with, upon reflection, seeming justice; I have heard the denunciation that systematists are prone to egotism, and that personal exploitation is the main object in their game! In this matter of the use of personal names in vernacular designation of species and subspecies I cite from *one* group, the jays: Couch's Jay, Woodhouse's Jay, Xantus's Jay, Belding's Jay, Steller's Jay, Semple's Blue Jay! What is there here to help any student to know what kind of a jay is concerned in each case? Human history, personalia, is surely *not* a first consideration in the study of systematic zoology. It does look as though the game of nomenclature were becoming more a matter of men and *their* names than of concepts of species and subspecies and of facts pertaining to *them*! Can't we stem this tendency? One important purpose of a general summation such as the A. O. U. Check-list is to be useful to present and future students in the earlier stages of their ornithological careers—to *new* students. The purpose should not be the acclaim of personal preferences or accomplishments on the part of the seniors in the field, or to impose these upon the juniors, save as these do, *in truth*, record advances in our science.

One more matter which will be deplored by many is the retention of possessives in personal vernaculars, as illustrated in the paragraph above. The majority of active bird students, at least in the West, prefer to speak of the Woodhouse Jay rather than say "Woodhouse's Jay". This is in accord with the commonest usage in other connections. We talk about the Hudson River, the Selkirk Mountains, San Francisco Bay, the Lincoln Memorial, the Douglas spruce, the Washington palm. Certainly the persons whose names have been employed in the naming of birds no more *own* those birds than, for instance, did David Douglas own the kind of spruce tree named after him! The next edition of the Check-list should take heed both of majority preference and good usage in this matter.

The new sequence adopted in the fourth edition has not been met by any renumbering of the species and subspecies. I think this was a wise decision—to avoid new numbers altogether, but to include the old ones, as is done, in brackets following each name. To facilitate the use of the new order, there is a concordance (pp. 473-478) of each old number with the page in the present edition where the name it concerns is to be found. Thus the numbering system employed in most collections of birds' eggs is not upset—it can be continued on the basis of the new list indefinitely. For Dr. Stone saw to it that even names of forms newly appearing in the present edition are given appropriate bracketed numbers.

As to the fossil record for North America, this has been brought down to date (indeed to June, 1931) and comprises a separate list of 156 extinct species, these in addition to ten forms of "sedis incertae" and many modern forms entered as from Pleistocene. The preparation of the fossil portion of the Check-list, we are told in

the Preface (p. xiv), was entirely the work of Dr. Alexander Wetmore. Advance in this field of ornithology is indicated by the fact that only 72 fossil species were listed in the third edition, in 1910.

The statements of range of species in the new edition have been entirely rewritten and many of them greatly expanded. As a whole, this feature thus shows marked advance over what it comprised in the third edition. But in the last twenty years there has, of course, been a huge increase in the volume of distributional data available; and I am not sure that the present edition is as satisfactorily a correct reflection of the present knowledge of the distribution of North American birds as the third edition was when *it* was issued. There is evidence that all published data have not been critically and discriminatingly scrutinized as they should have been; it looks as though in some cases "records" that have openly been pointed out as dubious have been taken at face value and probable errors thus perpetuated.

In the Preface (p. v) acknowledgment is made of the use made of the "extensive records" of the Bureau of Biological Survey. With no doubt whatsoever there has been accumulated under the auspices of that Bureau a far greater mass of data concerning the distribution of North American birds than exists in one place elsewhere. But those data are from miscellaneous and heterogeneous sources, many of them furnished by inexperienced observers. Exceeding acumen is required to sort such out, else mistakes will inevitably be entered in our literature. Very many of the "casual" and "breeding" records from those auspices need to be checked (and verified)—by an experienced distributional student—before their authenticity and significance can be accepted as reliable. I venture to say, from recent personal experiences in running down locality references of B. S. source, that many range designations in the fourth edition are more or less faulty, some of them seriously so. It is not, be it understood, here intended to cast discredit upon the services of the B. S. in amassing its huge store of distributional data. That store has enormous potential values and is immensely worth the effort expended. But the values in question cannot be derived from it without the exercise of a rather rigid standard of discrimination on the part of a talented geographical student; admittedly this would be an exceedingly difficult and laborious matter to have handled satisfactorily in connection with the Check-list.

Some of the seeming mistakes of a distributional character in the new Check-list, and one or more of which illustrate my point, are as follows. Of the Allen Hummingbird: "Has been recorded *casually* in migration in eastern and *southern* California", etc. [italics within quotes, here and below, mine]—when the bird really is numerous and regular in its migration through southern California, as the literature plainly shows. Of the Anna Hummingbird: "In *migration* casually to Arizona", etc.—whereas this species is not known to "migrate" at all. Of the Dwarf Horned Owl, *Bubo virginianus elachistus*: " . . . North to San Francisco Bay, California"—a range ascription never before suggested in print, to my knowledge. By the entire omission of the race *Otus asio inyoensis*, the Inyo region of eastern California lacks any Screech Owl of any kind. The type locality of the Arizona Verdin is given as "*northern* Lower California", instead of southern Lower California; and thereby the race *Auriparus flaviceps lamprocephalus* is wrongly given recognition.

Returning to the subject of ranges: Even at the risk of incurring a measure of opprobrium as a hypercritic, I feel that it might prove useful in future undertakings of the sort to point out in the new Check-list the comparative lack of any consistently followed order in the statement of distribution and of manner of occurrence. Highly desirable would have been uniform adherence to a plan of verbal composition and punctuation such as might have been previously worked out so as best to meet the

requirements of logic and quick understanding. The life-zone concept is employed in defining ranges (though unfortunately no life-zone map appears in this edition), but without uniformity. The limits of the normal breeding range are often not distinguishable because extreme marginal (often casual) occurrences are the ones cited. Indeed, casual or "accidental" occurrences are given undue prominence, whereby the much more important facts of regular geographic limitation are obscured if not completely buried.

There is a "Hypothetical List" (pp. 365-375) with 48 entries, consisting of (1) valid species the record of whose occurrence in North America is open to question; (2) forms described as distinct species or subspecies but now regarded as hybrids, color phases, or individual variations; and (3) alleged species figured on plates of Audubon and Wilson but which have not since been identified with any living birds. A useful feature is the inclusion also of all names [in brackets] which have appeared in "hypothetical" standing in the preceding editions, with an indication of what disposition has now been made of them. It is interesting to note that four of these former "hypotheticals" now appear in the regular list, namely, Common Black Duck, Rufous-necked Sandpiper, European Curlew, and Redshank. Eleven species in regular standing in the third edition have been transferred to hypothetical status in the fourth edition.

A detailed "Summary of Changes, Additions and Eliminations in the Present Edition of the Check-list as Compared with the Third Edition" provides an analysis which well repays the reading of it. One thing of interest showing here, is the list of 41 forms named binomially in the last edition and which now bear trinomial names.

The volume closes with an Index of 48 pages and close to 2200 entries. This no inconsiderable task in itself was in part the work of Dr. T. S. Palmer, as acknowledged in the Preface. But even with the helps such as this one, from various fellow committeemen, the entire undertaking of bringing out the fourth edition, involving probably double the effort put by Dr. Allen on the third edition, constituted a huge task on the shoulders of one man, the Chairman of the Committee. Indeed this might aptly be called the *Stone* Check-list of North American Birds.

One further comment: It is doubtful in my mind if any formally constituted committee ought ever again undertake this particular job—better it be the responsibility of one man (as in the case of Peters' list of the birds of the world), with such aid as he chooses to elicit from other individuals. A committee is too prone to be ruled by the fiat of some one aggressive individual who happens to be guided in his decisions by his own "judgment" [which too often equals "hunch"!]. Also, no one person takes the laborious responsibility of looking up a queried case for himself, each leaving it to the other fellow to look up or else assuming that some other committee member *will* or *has* looked it up. It is humanly impossible for a committee to do as thoroughly any work involving a great amount of detail as a single competent man who feels the entire burden of responsibility on his own shoulders. This I have seen demonstrated over and over again in the behavior of committees. Indeed, it is often the case that the energies and time of five men are dissipated in argumentative debate led by some aggressive, talkative individual over some few questions of minor import, leaving many vital considerations untouched.

I may suggest finally that the alert, active, systematically inclined student will at once acquire *two* copies of the new Check-list, one to keep as a book, unmarked, the other to begin immediately to mark up in pencil—with corrections of typographical and factual mistakes, with emendations of ranges to accord with the results of critical examination he or others may make of the existing records, and with departures in all other respects from time to time as authoritative revisions are published. In the

interests of advancement of American ornithology, not to exceed ten years should be allowed to elapse before the appearance of the next, *fifth* edition of the A. O. U. Check-list. I have no sympathy with the often-heard plea for permanence and stability, save as these ideals may be achieved upon the firmest foundations of fact and correct interpretation; and these are slow of accumulation. The issuance of Check-lists at rather frequent intervals records the progress *toward* the ideal (of course, never actually realizable)—and in doing that it stimulates activity in our field of science.

We now come to pass comment upon the first installment of Peters' list of the birds of the world[2]. This first volume may be accepted as indicating the manner of treatment which will be followed throughout the ten or more volumes which will be required for completion of the undertaking. The last summary of the sort was R. Bowdler Sharpe's Hand-list of the Genera and Species of Birds, published by the British Museum, 5 volumes, 1899-1909, with a General Index issued in 1912. In reviewing this latter work, J. A. Allen (Auk, xxvii, 1910, pp. 93-95) pointed out that it was already, at the time he reviewed it, far behind the times, in point of sequence, in criteria for recognizing forms, and in other respects—though nevertheless of very great use to working ornithologists. Great extensions of knowledge have taken place since even 1910, and an inventory for the whole world has been more and more acutely needed of the later years.

Sharpe listed approximately 18,939 "species" (which, however, meant species plus subspecies, since he used only binomials). This, then, can be accepted as the figure for the World's known ornis in 1909. One is prompted to wonder what the figure now is. If we take Peters' volume I as basis, which lists approximately 1830 species and subspecies, then it can be figured that the conclusion of his labors fifteen years or so hence will show somewhere near 20,000 recognized forms as the total for the world. It must be recalled here that many of the "species" listed by Sharpe have proven to have no standing at all according to the modern point of view, so that the subsequent great activity in naming new forms must be discounted in making estimate of ultimate totals.

Giving attention now to the Peters volume: The phylogenetic sequence followed is thoroughly modern, exactly as in the new A. O. U. Check-list insofar as it goes—which is through the Falconiformes. This circumstance, of close concordance in arrangement in the two lists, is superlatively helpful to the student, enabling him to use both lists, one supplementing the other, in general systematic and museum work. Peters deals with modern birds only; no fossil species are listed, though the names of fossil orders and families are included in their proper places so as to complete the phylogenetic picture. Incidentally, a novel definition for "fossil" is offered (p. vii): "Any bird is considered as belonging among the fossils if it is not known from at least a fragment of the skin and feathers."

As contrasting with the A. O. U. Check-list, Peters omits any and all vernacular names. This, I believe, will detract seriously from the general usefulness of his list. There is little or no excuse on the basis of economy of space, since at least half of the name-line in most cases is blank! Advanced students in any field must have regard for their juniors; it is the plain duty of the former to employ aids, wherever feasible, toward the saving of time and mental energy on the part of future and younger students. Then Peters uses the spelling "Linné" instead of Linnaeus, this constituting

[2] Check-list | of Birds of the World | Volume I | by | James Lee Peters | Assistant Curator of Birds, Museum of Comparative Zoology | at Harvard College | [design] | Cambridge | Harvard University Press | 1931 [our copy received October 14, 1931]; 8vo (148 x 228 mm.), pp. xviii+345.

an exception to most current usage including that in the A. O. U. list. A Swede ought to know about Swedish history, so I referred to Erik Nordenskiöld's "The History of Biology" (Knopf, New York, 1929) finding there (on pp. 203ff) that Linnaeus (this the original, avowed spelling) was not ennobled (to *von Linné*) until well along in his career. Furthermore, on the title and introductory pages of the tenth edition of the Systema Naturae (1758), whence most of our Linnaean names date, there is no use anywhere that I can find of the spelling "Linné". Peters appears to have made the wrong choice in this matter.

On the other hand, in the matter of preserving precisely the original spelling, even to the double "*i*" where originally so spelt, Peters adheres without deviation to the rules of the International Code of Zoological Nomenclature. This is a wholly defensible course—which cannot be said of the A. O. U. list. The ranges of species in the Peters list are defined succinctly as of *normal* occurrence; casual records are not taken into account. This means that there is no fogging of the explicit distributional status of a species such as the student usually seeks to know. Commendable conservatism is shown in the recognition of genera; as an example, *Accipiter* is used for the Goshawk as well as the Sharp-shinned and Cooper hawks. *Astur* is not employed—as it is in the A. O. U. list. Also the Mallards, Teals and Pintails are all included under the one genus *Anas*.

There is a, to me, less fortunate tendency toward uniting New and Old World holarctic series of forms, all as subspecies within single species; as examples, the Scoters, Goshawks and Marsh Hawks. In some of these cases, intergradation may be well established, and therefore Peters' action right; but I doubt the propriety of putting the European and Green-winged teals as subspecies under one species as Peters does; and there are other equally dubious cases. The A. O. U. rulings in this regard seem most frequently correct. There are other sorts of difference between Peters' and the A. O. U. list—particular cases involving specific versus subspecific status or the employment of different names for the same form. In most of these cases, the pro's and con's have been thrashed out in preceding literature; it will suffice here merely to cite some of them.

Peters uses the name *Haliaeetus leucocephalus washingtoniensis* (Audubon), based on a young bird from Kentucky, for the Northern Bald Eagle. The A. O. U. uses the name *H. l. alascanus* C. H. Townsend, based on an adult bird from Unalaska, for the same subspecies. After reviewing this case, I concur with the A. O. U. For the Red-tailed Hawks, Peters uses the name *jamaicensis* Gmelin (1788) as the middle element in the series of names, rather than *borealis* Gmelin (1788, one inch farther down the same page!); thus, *Buteo jamaicensis kriderii* instead of *Buteo borealis kriderii,* for the Krider Hawk. But the brief description Gmelin gives under the name *jamaicensis* is so vague, applicable perhaps to a young bird, while that under *borealis* is unmistakable, that again we concur with the A. O. U. ruling in favor of the use of the latter, as in literature generally heretofore. Incidentally, Peters does not recognize a western race of Red-tail at all, the name *calurus* being synonymized under *borealis*. Thus he calls the Red-tails of the whole breadth of the continent from Massachusetts to California, *Buteo jamaicensis borealis*. I would like to see a full revisionary study of the Red-tails (which has not been published upon, if such has been made) in support of this ruling, before accepting it. Some other cases which raise doubt are to be seen in Peters' treatment of the Sharp-shinned Hawks, the Scaup Ducks, the Brant, and the Snow Geese.

The Peters list includes a few forms, that I happen to know something about, whose tenability I doubt. *Buteo "jamaicensis" alascensis* Grinnell does not appear to

be separable from *calurus*. *Melanitta "fusca" dixoni* (W. S. Brooks) has not been clearly demonstrated as a good form. *Ardea herodias oligista* Oberholser was quite certainly based on an individual variant of *A. h. hyperonca*; anyway, there is no separate race of Great Blue Heron "resident" on the Santa Barbara Islands, as Peters indicates (see the clear statement of this case by A. B. Howell, Pac. Coast Avif., No. 12, 1917, pp. 43-44). And there are many other groups in which thorough, critical revision is needed, before the treatment, in Peters *or* in the A. O. U. list, can be thought of as approaching finality.

A very great value in periodic summations of the sort here under review lies in the stimulation they furnish for renewed attention to systematic groups whose arrangement or constitution come to appear unsatisfactory to students with increasing material and knowledge at their command. The value of the laborious services of such men as Sharpe, Stone and Peters cannot be justly appreciated save in the perspective which can develop only with a lengthening course of systematic history in view. Incidentally, these reflections, with other considerations, have led the present reviewer to wonder whether leadership in systematic zoology be not passing from the Old World to America.

Museum of Vertebrate Zoology, University of California, December 13, 1931.

THE SUMMER DISTRIBUTION OF CERTAIN BIRDS IN CENTRAL AND NORTHERN ARIZONA

By ALDEN H. MILLER

The summer of 1931 I spent in the Rocky Mountain and Great Basin regions for the purpose of studying breeding populations of Juncos. I was accompanied in the field by Mrs. Miller, and our expeditions to various mountain ranges in central and northern Arizona incidentally afforded opportunities to observe and collect birds of species other than Juncos. In view of the scanty information regarding the birds of some of the localities visited, it is appropriate to record certain of our findings. Eleven principal collecting stations were made in Arizona, but unfortunately our stay at each place was limited to four days or less. Rather than attempt to produce a full list of birds observed, I will confine comment to a few species which by reason of extension of range, breeding record or rarity seem to merit consideration.

Briefly, our itinerary was as follows: May 24-27, Soldier's Camp, elevation 7700 feet, Santa Catalina Mountains, Pima County; May 29-31, Stanley, elevation 4600 feet, Graham County; June 1-3, Pinal Mountain, elevation 7500 feet, Gila County; June 4-7, Workman Creek, elevation 5600 feet, Sierra Ancha, Gila County; June 8-10, Collum's Ranch, elevation 4000 feet, base of Mazatzal Mountains, 7 miles west of Rye Creek, Gila County; June 11, 12, Pivot Rock Spring, elevation 7000 feet, Mogollon Mountains near Baker Butte, Coconino County; June 13, Mingus Mountain, elevation 7500 feet, Yavapai County; June 14, 15, Mount Union, elevation 7500 feet, Bradshaw Mountains, Yavapai County; June 17-18, Sitgreaves Mountain, elevation 7500 feet, 8 miles north of Maine, Coconino County; June 22-24, 3 miles south of Dry Park Ranger Station, elevation 8000 feet, Kaibab Forest, Coconino County; June 25, 26, 4 miles north of Jacob's Lake, elevation 7200 feet, Kaibab Forest, Coconino County.

In the belief that vernacular names for subspecies are unnecessary as well as awkward, I have, as far as possible without creating new names, used common names representative of the entire species regardless of race. The numbers of the specimens listed are those of the Museum of Vertebrate Zoology.

Astur atricapillus atricapillus. Goshawk. Twice seen in the Kaibab Forest, once on June 24 in aspen and spruce timber near Dry Park and again in pure yellow pine timber four miles north of Jacob's Lake. At the latter place the bird came into the trees at camp at dusk and commenced squalling. It was plainly seen to be an adult, but no young could be found much as its actions suggested the presence of a nest or family. It is presumed that these birds belong to the eastern form, *A. a. atricapillus*, since a bird taken to the north in Utah proved to be of this race.

Accipiter cooperii. Cooper Hawk. A nest was found in a grove of yellow pines almost over our camp at Sitgreaves Mountain. On June 18 the nest contained one infertile egg, one pipped egg, and one young which had hatched the day preceding.

Aquila chrysaëtos canadensis. Golden Eagle. At Sitgreaves Mountain a fully grown young eagle was found occupying a nest and adjoining ledge on a 150-foot cliff within 300 yards of camp. The young bird yelped almost continuously during early morning and evening. Its curiosity was aroused by my presence at the base of the cliff to the extent that it came out on the ledge to peer over. Upon the approach of a parent carrying food it lost all interest in me and after loud cries soon became engrossed in tearing at the animal which had been brought.

Lophortyx gambelii gambelii. Gambel Quail. Seen in numbers even in the oak and chaparral belts north as far as Collum's Ranch. A single male was noted as it uttered its spring call at Phantom Ranch, Bright Angel Creek, Grand Cañon, on June 19. This point is several miles east of Cataract Cañon which Swarth (Pacific Coast Avifauna, No. 10, 1914, p. 22) gives as the eastern limit of this species in the northern part of the State.

Antrostomus vociferus arizonae. Whip-poor-will. Heard in the oak and pine forests on the Santa Catalina Mountains, on Pinal Mountain, where they were seen, and in the Sierra Ancha. Swarth (Condor, xx, 1918, p. 22) already has reported them north to the latter locality, but they also were found by us much farther to the north in the Bradshaw Mountains near Prescott on the night of June 13.

Sphyrapicus thyroideus nataliae. Williamson Sapsucker. Part of one of these woodpeckers was taken from the stomach of a female Cooper Hawk collected at Sitgreaves Mountain. In the Kaibab Forest they were the most abundant of the woodpeckers, their characteristic broken drumming, *ttttttt—tt—tt—tt—tt*, being heard everywhere. Two nests containing well grown young were found on June 22. Both nests were in dead trees twenty to twenty-five feet above ground. This sapsucker was present in pure stands of yellow pine at Jacob's Lake.

Picoïdes tridactylus dorsalis. Three-toed Woodpecker. Frequently met in the yellow pine and spruce forests of the Kaibab Plateau where a pair was collected: ♂, no. 58100; ♀, no. 58101.

Myiarchus tyrannulus magister. Arizona Flycatcher. This flycatcher, which usually is associated with the saguaro belt, was found at Stanley among sycamore trees in a cañon bottom. It also was met at Collum's Ranch north of the saguaro belt in the upper Tonto Basin. This bird is one of a number of low desert types noted by Swarth (Birds of the Papago Saguaro National Monument, etc., U. S. Dept. Int., 1920, pp. 56, 57) around Roosevelt Lake, which types range north along Tonto Creek into the oak and juniper belt in the vicinity of Rye Creek.

Empidonax wrightii. Wright Flycatcher. Although known as a migrant within the State, published evidence of breeding seems to have been lacking heretofore. A male, no. 58106, was taken June 22 near Dry Park and many others were seen. Nests were not located, but the actions of the birds and condition of the gonads of the bird collected indicate breeding in the Kaibab area.

Aphelocoma sieberi arizonae. Arizona Jay. Swarth (Condor, xx, 1918, p. 21) found these birds north as far as the Sierra Ancha where we also took specimens. However, they were found by us to occur north of this point at Collum's Ranch on June 9 and even as far as Payson, where on June 10, I watched a pair feeding a short-tailed juvenile in a clump of alligator junipers.

Corvus brachyrhynchos hesperis. Crow. The scarcity of the crow in the State makes it desirable to mention a single bird seen in the Sierra Ancha in pine timber at 6500 feet elevation on June 7, and a small flock noted in the live oaks near Collum's Ranch, June 9.

Penthestes gambeli gambeli. Mountain Chickadee. Found breeding in small numbers on the Santa Catalina Mountains and on Pinal Mountain. A single bird was seen on the summit of the Mazatzal Mountains in the yellow pines, June 9.

Baeolophus inornatus griseus. Plain Titmouse. Records of this titmouse are few for south-central Arizona. Brewster (Bull. Nutt. Orn. Club, vii, 1882, p. 79) reported it from the Chiricahua Mountains, and more recently L. Miller, W. P. Taylor and H. S. Swarth (Condor, xxxi, 1929, p. 77) recorded a specimen taken November 28, 1928, at the base of the Santa Catalina Mountains. I collected a male, no. 58114, of a pair encountered in the junipers near Stanley, May 30, 1931. The birds evidently had young in the vicinity. No other examples of *inornatus* were seen in this part of the State; *B. wollweberi* was common.

Heleodytes brunneicapillus couesi. Cactus Wren. Swarth noted this wren at Roosevelt. We found it here and also north as far as Collum's Ranch in the opuntia cactus on open hillsides.

Oreoscoptes montanus. Sage Thrasher. This thrasher apparently was breeding in the artemisia brush south of Fredonia near the edge of the Kaibab Forest, inasmuch as it was present there on June 26. Breeding season records for the State were lacking heretofore.

Turdus migratorius propinquus. Robin. Present throughout the pine forest on the Santa Catalina Mountains where evidently nesting. A nest containing four pipped eggs was found in a yellow pine tree above our camp on Pinal Mountain on June 1. It is difficult to believe that robins could have been overlooked by previous visitors to the Santa Catalina Mountains during the summer months, yet this range is not mentioned as a breeding station by Swarth (Pacific Coast Avifauna, No. 10, 1910,

p. 80). Scott (Auk, v, 1888, p. 167) evidently did not find them breeding there in 1885 and 1886. Possibly they have since then commenced nesting in these mountains; they have long been known to winter in this general territory.

Hylocichla guttata auduboni. Hermit Thrush. Abundant and breeding on the Santa Catalina Mountains, contrary to what might have been expected from Scott's statement (Auk, II, 1885, p. 349) that he found only one pair of these birds in late April of 1885 on this range. Three males were heard singing at different points in the dense timber of the northeast slope of Pinal Mountain on June 2.

Polioptila caerulea amoenissima. Blue-gray Gnatcatcher. Seen occasionally in the chaparral at both Stanley and Collum's Ranch.

Corthylio calendula calendula. Ruby-crowned Kinglet. Abundant throughout the spruce belt of the Kaibab Forest in the latter part of June. Its presence here in summer was to be expected although it does not seem to have been recorded before.

Vireo huttoni stephensi. Hutton Vireo. This vireo was met at Collum's Ranch in the large live oaks in the cañon bottoms. A singing male, no. 58144, was taken June 9 and a fully grown juvenile, no. 58145, on June 10. Both are typical of *stephensi*.

Vireo vicinior. Gray Vireo. Very abundant in the juniper and scrub oak cover of the hillsides at Stanley, occasionally invading the live oaks in the cañon bottoms where they sang in the same trees with Solitary Vireos, specimens of both having been taken. Gray Vireos and Black-chinned Sparrows occurred in the same brush patches much as they often are associated in parts of southern California. Gray Vireos also were present at Collum's Ranch.

Vireo gilvus swainsonii. Warbling Vireo. This vireo, supposedly of limited occurrence in southern Arizona during the summer months, was fairly common in the deciduous oaks on the Santa Catalina Mountains and on Pinal Mountain.

Vermivora virginiae. Virginia Warbler. In addition to mountain ranges from which this form is already known during the summer, we encountered it on Pinal Mountain, the Mazatzal Mountains and Mingus Mountain.

Vermivora luciae. Lucy Warbler. Found ranging north and east into the Upper Sonoran oak belt at Stanley, where a bird, no. 58148, was collected in a grove of sycamores, and at Collum's Ranch where they were present in the live oaks.

Dendroica aestiva sonorana. Yellow Warbler. A breeding male, no. 58151, typical of this race, was taken at Collum's Ranch, June 9.

Dendroica graciae graciae. Grace Warbler. This warbler was breeding commonly in pure stands of yellow pines on most of the mountain ranges visited. They were present on Pinal Mountain and the Mazatzal Mountains, localities not specifically mentioned before. More unusual, however, was their presence in numbers four miles north of Jacob's Lake in the Kaibab Forest where a breeding female, no. 58153, was secured on June 25. Previously they have not been reported north of the Grand Cañon. A nest was found on June 5 at about 6500 feet elevation in the Sierra Ancha. It was twenty feet above ground in the terminal tuft of needles of a low side branch of a yellow pine. Both parents were occupied feeding large-sized young.

Icterus cucullatus nelsoni. Hooded Oriole. Found at Stanley on May 29 with nest and young and at Collum's Ranch on June 9. Swarth found the species at Roosevelt Lake in 1917.

Icterus parisorum. Scott Oriole. Rare at Collum's Ranch. North of here only one bird was noted; this was seen in the vicinity of Monte Rio on the juniper-covered plateau about twenty-five miles north of Prescott, June 15.

Richmondena cardinalis superba. Cardinal. Found to extend north of Roosevelt Lake, where noted by Swarth, to Collum's Ranch where at least two pairs were seen on June 9. A female, no. 58162, accompanied by two short-tailed juveniles was collected in the chaparral at Stanley, May 30.

Passerina amoena. Lazuli Bunting. Two males singing continuously and seemingly stationed for breeding, on June 9 at Collum's Ranch.

Hesperiphona vespertina warreni. Evening Grosbeak. A pair was taken on the Sierra Ancha, June 5, the female possessing a brood patch and having laid a short time previously. These birds are interesting since they come from a region between the ranges of *warreni* and *montana* as defined by Grinnell (Condor, XIX, 1917, pp. 20, 21). *Warreni* is recorded by him from Fort Verde to the north and *montana* from the Chiricahua Mountains to the south. The male which I collected, no. 58164, the only bird preserved as a skin, has a decidedly stouter bill than any specimen of *mon-*

tana in comparable plumage which I have examined. It also shows a broad frontal bar which is characteristic of *warreni*. I do not have material available to warrant comment upon the validity of *warreni* as distinct from *brooksi* of the more northern Rocky Mountains. However, the Sierra Ancha bird clearly belongs to the central Rocky Mountain form and I can see no approach in characters to *montana*. Evening Grosbeaks were common in the Kaibab area.

Loxia curvirostra ssp. Crossbill. On May 26 on the Santa Catalina Mountains, a flock of a dozen Crossbills was feeding near camp and some of the birds singing. On June 1 and 2 on Pinal Mountain they also were noted. These occurrences at this season hardly are evidence of breeding in a bird of such nomadic habits and irregularity as regards breeding; nevertheless it is not unlikely that they nest in the localities mentioned. It is assumed that the birds above noted were *L. c. stricklandi*.

At Jacob's Lake, Kaibab Forest, a male, no. 58337, not breeding, taken June 25, is *L. c. bendirei*, as also are several specimens taken in southern Utah.

Junco caniceps dorsalis. Gray-headed Junco. Pending a full report upon the Juncos of this region, I will at present mention only the discovery of breeding birds on Mingus Mountain and on the Bradshaw Mountains. Both these ranges are south and west of the previously known breeding range of this form in the Mogollon Mountains and on Bill Williams Mountain. They were decidedly rare on both Mingus and the Bradshaw mountains, as one specimen only was taken in each place (numbers 58198 and 58199, respectively) after considerable effort. I place *dorsalis* as a subspecies of *caniceps* because intergradation between the two was found in the Kaibab Forest. The forms *dorsalis* and *J. phaeonotus palliatus* possess distinguishing characters in the color of eye and bill which are constant and do not intergrade, at least in Arizona.

Spizella atrogularis atrogularis. Black-chinned Sparrow. About eight males were heard singing in the scrub oak brush at 5000 feet elevation near Stanley on May 30. This species also was present near Collum's Ranch, June 9, on brush covered hillsides. Birds collected are of interest since they materially add to the number of specimens of *S. a. atrogularis* which were available for comparison when Grinnell and Swarth (Auk, XLIII, 1926, pp. 475-478) demonstrated the existence of the race *cana* The measurements in millimeters of my specimens are as follows:

				Wing	Tail	Exposed culmen	Tarsus
No. 58351	♂	Stanley	May 30, 1931	69.0	80.0	8.9	1.85
No. 58352	♂	Collum's Ranch	June 9, 1931	63.4	70.5	9.1	1.86
No. 58353	♂	Collum's Ranch	June 9, 1931	63.3	70.7	9.1	1.92
No. 58350	♀	Stanley	May 30, 1931	60.2	68.9	9.1	1.85

Comparisons with measurements given by Grinnell and Swarth (*op. cit.*, p. 477) and by A. H. Miller (Condor, XXI, 1929, p. 207) in connection with the description of *S. a. caurina* show that the size differences between *cana* and *atrogularis* are as marked in these specimens as in those previously available for examination. The long tail of no. 58351 is unusual even for the long-tailed race *atrogularis*. The small series above listed is appreciably less brown than *cana*. They also are consistently lighter above and below than *caurina*, particularly as regards the pileum. The wing and tail of *caurina* and *S. a. atrogularis* are similar as to length, but the culmen and tarsus measure shorter in *caurina* than in *atrogularis*, as before pointed out. A juvenile, no. 58354, the offspring of no. 58353, is markedly lighter on the pileum and back and much less streaked beneath than the juvenile of *caurina*. The *S. a. atrogularis* juvenile is grayer, that is, less brown, above than juvenal *cana*.

Museum of Vertebrate Zoology, Berkeley, California, October 12, 1931.

FROM FIELD AND STUDY

The Black Swift in Glacier National Park.—The Black Swift (*Nephoecetes niger borealis*) appears to be a rare bird in Montana. No record of its occurrence is given by Saunders in his list of Montana birds (Pac. Coast Avif. No. 14, 1921). In "Wild Animals of Glacier National Park: The Birds" (Nat. Park Service, 1918), Mrs. F. M. Bailey gives no record for this species, but indicates (p. 106) a possibility of its being seen on rare occasions. More recently I have published records of its occurrence at Libby (Auk, XLVII, 1930, p. 98) and in Glacier Park (*ibid.*, p. 98, and Bird-Lore, XXXIII, 1931, p. 175).

During the summer of 1931, I visited Glacier Park on four occasions, and observed Black Swifts twice. During a one-day visit on June 7, I observed three of the birds flying above McDonald Creek near the mouth of Avalanche Creek. On August 17, I traversed the length of the Garden Wall on the trail from Logan Pass to Granite Park, climbing to the summit twice, and descended from Granite Park to McDonald Creek Valley, without seeing a Black Swift. The next morning, however, I saw four birds of this species near Avalanche Lake. On two occasions later in the summer I was in Glacier Park for a few hours, but did not observe any Black Swifts.

The new A. O. U. Check-list (p. 177) questions the inclusion of Montana in the breeding range of the Black Swift. The records cited and described above indicate that it probably breeds sparingly in Glacier National Park.—WINTON WEYDEMEYER, *Fortine, Montana, January 14, 1932.*

A Station of Frequent Observation of the Cowbird in the San Francisco Bay Region.—The lower stretches of Coyote Creek between San Jose and San Francisco Bay provide an attractive region for the Cowbird (*Molothrus ater*, subspecies?). The stream here flows through the rich alluvial soil of the lower Santa Clara Valley, has built up the dikes characteristic of California streams in such places, and these in turn provide a luxuriant growth of cottonwoods, box elders, red willows and other trees, with a rank undergrowth of brambles, poison hemlock and other coarse herbs. Such a condition provides ideal breeding grounds for many small birds such as the Russet-backed Thrush, Song Sparrow, Traill Flycatcher, Willow Goldfinch, Pileolated Warbler and Yellow-throat. These breeding birds in turn provide hosts for the Cowbird. It was in this region that Smith (Condor, XXVIII, 1926, p. 245) located eggs of the Cowbird but did not observe adult birds. Since that time adult Cowbirds have been observed with some regularity on the Coyote about two miles above its entry into the salicornia marshes of the Bay and in the vicinity of the bridge over this stream on the cross-road connecting the towns of Alviso and Milpitas.

Males and females have been observed. The males have been clearly seen in gurgling flight and in full song; and at one time three of them sat and performed on telephone wires immediately over the heads of a group of us. Dates before me of actual observation of adult Cowbirds are as follows: July 8, 1930, one male in flight; July 19, 1930, three males immediately above our heads on telephone wires, one male in full song on a box elder tree; May 9, 1931, a pair; May 14, 1931, notes of two or three and a pair (male and female together); July 28, 1931, male, singing.—GAYLE PICKWELL, *State College, San Jose, California, November 4, 1931.*

Note on the Food of an Arizona Spotted Owl.—The Arizona Spotted Owl (*Strix occidentalis lucida*) may be classed as one of the rarer North American owls. In fact the references in literature to specimens collected or observed are few, while notes on this owl's food habits are almost lacking. E. S. Steele in 1927 recorded (Condor. XXIX, 1927, p. 123) the capture of a young Arizona Spotted Owl near Reserve, New Mexico, and his observations upon its behavior. This owl readily preyed on the rats and mice while confined in a barn, and willingly accepted dead squirrel, chipmunk, rat or mouse from its captor's hand. So far as Mr. Steele was able to observe, its diet was entirely mammalian, the flesh of birds being scorned.

In contrast to these observations, there are records (Richardson, Condor, VIII, 1906, p. 57; Daggett, Condor, XV, 1913, pp. 40-41) of two Spotted Owls (*Strix occidentalis occidentalis*) from the mountains in the vicinity of Los Angeles, California, that were cannibalistic. Each of the birds had eaten a Pigmy Owl (*Glaucidium gnoma californicum*) shortly before it was collected.

Another and entirely different type of food was found by the writer when he skinned and dissected an adult male Arizona Spotted Owl, which was secured at Rustler Park, Chiricahua Mountains, Arizona, on June 23, 1931, by E. H. Quayle, one of a party making collections in that region for the San Diego Society of Natural

History. A cursory field examination of the bird's stomach revealed that it was filled to distention with insects; so it was sewn up and preserved in alcohol. Later, the contents were segregated in the laboratory, and sent to the United States Bureau of Biological Survey, where, through the kindness of Mr. W. L. McAtee, it was determined that they consisted entirely of parts of at least seventeen noctuid moths of the genus *Agrotis*. There were also seven feathers in the mass, which, when dried and fluffed, proved to be breast feathers from the owl. This may have been due to the bird's preening itself, for it was molting and in very poor feather.

This specimen was killed from its perch in a fir tree about midnight by the aid of a powerful electric flash-light. This fact may have some bearing on the discovery of its insect diet, for there seems but little question that fragile fare of this sort would pass quickly through the digestive tract; and had the bird been killed from its day roost, its stomach would very likely have been found to be empty. This was the writer's former experience with spotted owls (Condor, XV, 1913, p. 229).—LAURENCE M. HUEY, *San Diego Society of Natural History, Balboa Park, San Diego, California, September 24, 1931.*

Bird Notes from the Upper Rio Grande Valley.—In June, 1930, I had an opportunity to study the waterfowl of the Rio Grande Valley at Socorro, New Mexico. Later, after reference to "Birds of New Mexico" and correspondence with Mrs. Florence M. Bailey, it was thought advisable to publish records of the ducks seen. Breeding localities and summer records for ducks in the state are not numerous and breeding grounds are confined mainly to lakes in Rio Arriba County and areas of the lower Pecos Valley. The following notes may be of interest.

New Mexican Duck (*Anas diazi novimexicana*). This duck is restricted in range, being found only in the Rio Grande Valley of New Mexico from Albuquerque south to El Paso, Texas. Seven of these black ducks were seen on a pond near the city limits of Socorro on June 12, 1930.

Cinnamon Teal (*Querquedula cyanoptera*). Five seen in a slough along the Rio Grande on June 12. This species has been recorded from many localities in the state but is not particularly abundant anywhere.

Shoveller (*Spatula clypeata*). This duck nests at Lake Burford, Rio Arriba County, and in the Pecos Valley near Roswell. I found a male and female on the Rio Grande, June 11.

Lesser Scaup (*Nyroca affinis*). Considered by Mrs. Bailey as a migrant and rare winter resident in the state. These birds were found in May and June, 1918, at lakes of northern New Mexico by Dr. Wetmore, but no definite nesting records were given. Near Socorro on June 12, 1930, I discovered a pair of these ducks but was unable to find proof that they were nesting there.

Ruddy Duck (*Erismatura jamaicensis rubida*). According to "Birds of New Mexico" this duck nests commonly in lakes in the northern part of the state, in the Pecos Valley and on the Carlsbad Bird Reserve. I found, on June 12, a female with six young, only a few days old, in a slough near Socorro.

On August 5, 1931, I was in El Paso, Texas, and was able to spend some time studying a colony of Great-tailed Grackles (*Megaquiscalus major macrourus*) along the river at the south side of the city. Several of the birds observed were young of the season. I was interested to find that this bird is rare in New Mexico and western Texas. All records given for New Mexico are from the southern part of the state along the Mimbres and Pecos rivers and the Rio Grande. One breeding colony was found in 1925 near Carlsbad, and birds have been taken at Las Cruces and have bred at La Mesa, New Mexico, thirty miles north of El Paso.—JAMES STEVENSON, *Los Angeles, California, December 21, 1931.*

The Wood Ducks in Napa County, California.—In the pioneer days the Wood Duck (*Aix sponsa*) was reported to have been exceedingly abundant in the upper reaches of the Napa River. The writer had never seen any here until the spring of 1925, when a flock of twenty-six was reported to be wintering in a small lake-like slough near the "Little Trancas." On March 6, 1925, I accompanied Dr. Harold C. Bryant to this spot, and at 6 p. m. Dr. Bryant identified three Wood Ducks which dropped into the water. A flock of pinioned domesticated mallards is now kept by an orchardist at this place, and a "large" number of Wood Ducks was reported there this fall. On November 12, between 5:30 and 6:00 p. m., the writer and Eagle Scout Wayne Boggs observed a flock totalling 60 or more which came in to the slough, and on November 13, I counted 48 individuals.—E. L. BICKFORD, *Napa, California, November 21, 1931.*

EDITORIAL NOTES AND NEWS

The seventh annual meeting of the Cooper Ornithological Club is scheduled to take place in the Los Angeles area on April 22 and 23, 1932. The regular meeting of the Board of Governors will follow on the twenty-fourth. While goodfellowship and better personal acquaintance are important ends served by these general meetings, the scientific papers and the opportunity for discussion of papers constitutes the fundamental element. The six previous occasions of meeting have done their service well. The 1932 meeting is expected to be quite as full of constructive discussion. The building of the scientific program is not a "one man job," although the final arrangement of its separate items is properly so delegated. The papers are submitted by the Club members at large, and stimulus or outlet for such contribution is the major function of the annual meeting. Let us each one take that thought seriously. The one man function, however, will be greatly facilitated if each contributor who submits a title will at the same time indicate the nature of his contribution. A sentence or two in explanation sometimes assists greatly where a title is hard to "translate". We sometimes suspect the title in fiction or in drama of being in the nature of *concealing coloration*. Might not our scientific titles more properly strive to be *revealing colors*? What is your title? Send your reply to LOYE H. MILLER, *University of California at Los Angeles.*

Dr. Gayle B. Pickwell, whose recently published ecological study of the Prairie Horned Lark is reviewed at length and altogether favorably by Mr. McCabe in this issue of the *Condor* (p. 106), is a graduate from Cornell University who came to California some five years ago to teach in the natural science department in the San Jose State Teachers College. He has not only established his record as an extremely successful teacher there, but has put out a series of nature bulletins under the title Western Nature Study, in which animal and plant natural history is set forth on a high plane of factual worth and at the same time in a form for the immediate use of teachers. He is aided in this type of work through his marked ability in the field of wild-life photography. Dr. Pickwell is also active in promoting interest in birds through the Audubon Societies and in conducting summer courses in ornithology. Recently he was elected Vice-President of the Northern Division of the Cooper Ornithological Club.

Mr. George Willett, of the Los Angeles Museum, Exposition Park, Los Angeles, is undertaking a complete revision of his "Birds of the Pacific Slope of Southern California", originally published as Pacific Coast Avifauna Number 7. He urges that all important unpublished records for that region either be turned in to him for inclusion in his new compendium, or else be published independently so as to become generally available.

The first book on birds to reach us in the new year was Arthur H. Howell's "Florida Bird Life", published by the Florida Department of Game and Fresh Water Fish in coöperation with the Bureau of Biological Survey, United States Department of Agriculture (cap 4to [185 x 248 mm.], pp. xxiv + 579, 58 pls., 72 text figs. [maps]; to be had from the Florida Department of Game and Fresh Water Fish, Tallahassee, for $6.00). This is essentially a state list, well and authoritatively annotated, embellished with some general matter and an abundance of illustrations, part of these being from colored drawings by Francis L. Jaques. The typography throughout is excellent. The chapter giving the "History of Florida Ornithology" proved of special interest to us. And well worth studying because of its bearing upon current conservation problems is the chapter contributed by Robert W. Williams setting forth the "History of Bird Protection in Florida". There is a complete and accurate "Bibliography of Florida Ornithology" to which abundant reference is made in modern approved style, throughout the text. In this book, as a result of Howell's 12 years of consistent work upon it, we are now afforded the latest word as to the status of the many birds peculiar to that peninsula and so well known in the general literature of North American birds. In general it would seem that despite large reclamation movements the fortunes of the bird life of Florida are somewhat improving, due to the rising regard for bird life on the part of the citizens of that state.—J. G.

Dr. Casey A. Wood's "Introduction to the Literature of Vertebrate Zoology" (Oxford Univ. Press, London: Humphrey Milford, 1931, foliopost 4to [8½ x 11 inches], pp. xx + 643, frontispiece) pertains so extensively to ornithology that no working ornithological library can afford to do without it. The result of laborious bibliographic research, this work becomes at once useful to active students through the various classifications it gives—chronological, by groups of animals dealt with, by subject, and, exhaustively, by author. Of readable and extremely informative character are chapters on such subjects as Linnaean literature, travelogues of explorers, the literature of zoogeography, periodicals and serials, and rare and unique works. We congratulate Dr. Wood on the final completion of this monumental undertaking.—J. G.

Mr. John R. Pemberton got back on February 10 from a ten weeks' cruise along the west coast of Mexico, visiting the many islands from the head of the Gulf of California south to Socorro. Dr. William H. Burt and Mr. A. J. van Rossem were guests of Mr. Pemberton on his yacht, collecting vertebrates in the interests of Mr. Donald R. Dickey of the California Institute of Technology.

CURRENT DISCUSSION

GAME AND WILD LIFE CONSERVATION

This is a reply to Mr. T. T. McCabe's well written and persuasive *exposé* of two recent manifestations of the sportsman's movement: my "Game Survey of the North Central States," and the several publications issued by "More Game Birds in America." Both are, I take it, inclusively condemned as "a framework of pernicious doctrines, too often speciously glossed over."

Mr. McCabe's attitude raises what seems to me a fundamental issue. I hope that it may provoke some badly needed cerebration among both protectionists and sportsmen, and especially among those intergrades like myself, who share the aspirations of both.

There are many sportsmen who laugh at any attempt to embody the protectionist point-of-view in any game program. "Whatever you do the protectionists will be against it." Mr. McCabe's paper furnishes scant comfort to those of us who have been holding out against this attitude, because we see in it the indefinite continuation of the present deadlock, from which the sharpest pens gain much glory, but the game gains nothing except a further chance to disappear.

"More Game Birds" on the one hand, and the "Game Survey" (as further developed in the "American Game Policy") on the other, represent the opposite wings of the sportsman's camp. From their very inception they agreed to disagree on the very issues with respect to which Mr. McCabe presumably finds them both "pernicious," namely: predator control, exotics, degree of commercialization, and artificial propagation. This divergence, great enough to seem fundamental to two groups of hardened sportsmen, would, I had hoped, be perceptible to readers of the *Condor*.

I do not imply that Mr. McCabe should agree with either "More Game Birds" or myself on these moot questions. I ask, though, whether it is good for conservation for him to dismiss both, with one breath, as equally subversive of what he considers sound policy. (I think this is not too strong a statement, since Mr. McCabe says "these proposals are an offer . . . to the nation, for its game birds," to which he would reply, "Not for sale.")

Of course, no disagreement is ever as simple as it looks on paper. A partial explanation of this one lies, I think, in the fact that Mr. McCabe's game policy, whether he realizes it or not, consists of a system of *personal wishes* which might be realized if America consisted of 120 million ornithologists, whereas mine is a system of *proposed public actions* designed to fit the unpleasant fact that America consists largely of business men, farmers, and "Rotarians," busily playing the national game of economic expansion. Most of them admit that birds, trees, and flowers are nice to have around, but few of them would admit that the present "depression" in waterfowl is more important than the one in banks, or that the status of the blue goose has more bearing on the cultural future of America than the price of U. S. Steel.

Now if Mr. McCabe and I had the courage to challenge this universal priority for things material and things economic, we might consistently hoist the banner "Not For Sale" and die heroically under the heels of the mob. But have we not already compromised ourselves? I realize that every time I turn on an electric light, or ride on a Pullman, or pocket the unearned increment on a stock, or a bond, or a piece of real estate, I am "selling out" to the enemies of conservation. When

I submit these thoughts to a printing press, I am helping cut down the woods. When I pour cream in my coffee, I am helping to drain a marsh for cows to graze, and to exterminate the birds of Brazil. When I go birding or hunting in my Ford, I am devastating an oil field, and re-electing an imperialist to get me rubber. Nay more: when I father more than two children I am creating an insatiable need for more printing presses, more cows, more coffee, more oil, and more rubber, to supply which more birds, more trees, and more flowers will either be killed, or what is just as destructive, evicted from their several environments.

What to do? I see only two courses open to the likes of us. One is to go live on locusts in the wilderness, if there is any wilderness left. The other is surreptitiously to set up within the economic Juggernaut certain new cogs and wheels whereby the residual love of nature, inherent even in "Rotarians," may be made to recreate at least a fraction of those values which their love of "progress" is destroying. A briefer way to put it is: if we want Mr. Babbitt to rebuild outdoor America, we must let him use the same tools wherewith he destroyed it. He knows no other.

I by no means imply that Mr. McCabe should agree with this view. I do imply that to accept the economic order which is destroying wild life disqualifies us from rejecting any and all economic tools for its restoration, on the grounds that such tools are impure and unholy.

With what other than economic tools, for instance, can we cope with progressive eviction of game (and most other wild life) from our rich agricultural lands by clean farming and drainage? Does anyone still believe that restrictive game laws alone will halt the wave of destruction which sweeps majestically across the continent, regardless of closed seasons, paper refuges, bird-books-for-school-children, game farms, Izaak Walton Leagues, Audubon Societies, or the other feeble palliatives which we protectionists and sportsmen, jointly or separately, have so far erected as barriers in its path? Does Mr. McCabe know a way to induce the average farmer to leave the birds some food and cover without paying him for it? To raise the fund for such payment without in some way taxing sportsmen?

I have tried to build a mechanism whereby the sportsmen and the Ammunition Industry could contribute financially to the solution of this problem, without dictating the answer themselves. The mechanism consists of a series of game fellowships, set up in the agricultural colleges, to examine the question of whether slick-and-clean agriculture is really economic, and if not, to advise farmers how they can, by leaving a little cover and food, raise a game crop, and market the surplus by sale of shooting privileges to sportsmen. This mechanism is, I take it, specious. Have the protectionists a better one to offer?

Another mechanism which I have tried to build is the committee of sportsmen and protectionists charged with setting forth a new wild life policy. Has Mr. McCabe read it?

These things I have done, and I make no apology for them. Even if they should ultimately succeed, they will not restore the good old days of free hunting of wholly natural wild life (which I loved as well as Mr. McCabe), but they may restore something. That something will be more native to America, and available on more democratic terms, than "More Game Birds" pheasants, even though it be less so than Mr. McCabe's dreams of days gone by.

Let me admit that my cogs and wheels are designed to perpetuate wild life to shoot, as well as wild life to look at. This is because I believe that hunting takes rank with agriculture and nature study as one of three fundamentally valuable human contacts with the soil. Secondly, because hunting revenue offers the only available "coin of the realm" for buying from Mr. Babbitt the environmental modifications necessary to offset the inroads of industry.

I admit the possibility that I am wrong about hunting. The total cessation of it would certainly conserve some forms of wild life in some places. Any ecologist must, however, admit that the resulting distribution and assortment of species would be very irregular and arbitrary, and quite unrelated to human needs. The richest lands would be totally devoid of game because of the lack of cover, and the poorer lands nearly so because of the lack of food. The intermediate zones might have a great deal of game. Each species would shrink to those localities where economic accident offered the requisite assortment of environmental require-

ments. That same condition—namely the fortuitous (as distinguished from purposeful) make-up of wild life environments—shares, with overshooting, the credit for our present deplorable situation.

The protectionists will, at this point, remind me of the possibilities of inviolate sanctuaries, publicly owned, in which habitable environments are perpetuated at public expense. Let us by all means have as many as possible. But will Mr. Babbitt vote the necessary funds for the huge expansion in sanctuaries which we need? He hasn't so far. It is "blood money" which has bought a large part of what we have. Moreover, sanctuaries propose to salvage only a few samples of wild life. I, for one, demand more. I demand of Mr. Babbitt that game and wild life be one of the normal products of every farm, and the enjoyment of it a part of the normal environment of every boy, whether he live next door to a public sanctuary or elsewhere.

Mr. McCabe taxes me with omitting any mention of game production on public lands, where the one-gallus hunter will have free access to it. I can only infer that he has not read the American Game Policy. Has any group ever proposed a larger public land program, and called for more wild life production thereon? The Policy admits, to be sure, the unpleasant fact that lands must be cheap in order to be public. It advocates the paid-hunting system only for those lands too expensive for the public to own.

Finally Mr. McCabe taxes me with too much interest in exotics. Modesty forbids me to refute this charge in detail. I have persuaded two states to go out of the pheasant business, and several others to limit it to half their area. I devised the "glaciation hypothesis" which seems to exclude pheasants from about a third of the United States. On the other hand, I have recommended the continuation of pheasants and Hungarians in certain regions where economic changes have so radically altered the environment as to make the restoration of native game prohibitive in cost. Just what native species would Mr. McCabe recommend for east-central Wisconsin, or for northern Iowa, or for farm land in Massachusetts?

Let it by no chance be inferred that because I speak as a sportsman I defend the whole history of the sportman's movement. Hindsight shows that history contains any number of blunders, much bad ecology, and not a few actions which must be construed as either stubbornness or hypocrisy. For every one of these, one could point out a counterpart in the history of the protectionists, only there has been no "Emergency Committee" with either the means or the desire to compile and advertise them. Fifteen years ago, for instance, the protectionists closed the prairie chicken in Iowa, and then sat calmly by while plow and cow pushed the species almost to the brink of oblivion. Was this a blunder? Yes—but what of it? Is there any human aspiration which ever scored a victory without losing to some extent its capacity for self-criticism? The worthiness of any cause is not measured by its clean record, but by its readiness to see the blots when they are pointed out, and to change its mind. Is there not some way in which our two factions can point out each other's sophistries and blunders, without losing sight of our common love for what Mr. Babbitt is trampling under foot? Must the past mistakes of each group automatically condemn every future effort of either to correct them?

To me, the most hopeful sign in the sportsman's movement is that several little groups have publicly avowed that the old program is a failure. Each is struggling to devise a new formula. I am conceited enough to believe that the formula my little group is trying to put together comes as near meeting the ugly realities of economics on the one hand, and the ideals of the protectionists on the other, as any yet devised. Mr. McCabe's paper will neither help nor hinder its future acceptance or rejection among sportsmen, but it may hinder its thoughtful consideration by the protectionist camp, and thus prevent what I had devoutly hoped for: their active participation in its development, modification and growth.

Lest this be construed as an idle boast, let me point out that as chairman of the Game Policy Committee, I asked the A. O. U. to appoint a representative to sit on or with the Committee, and to pull the reins whenever the Committee got into proposals subversive of the protectionists' point of view. He has not yet pulled. I hereby invite Mr. McCabe to sit with him.

In short, I beg for a little selectivity in weighing the new departures proposed by the other fellow. I also pray for the day when some little group of protectionists will publicly avow that their old formula of restriction is not the whole

Alpha-to-Omega of conservation. With both sides in doubt as to the infallibility of their own past dogmas, we might actually hang together long enough to save some wild life. At present, we are getting good and ready to hang separately.—ALDO LEOPOLD.

PUBLICATIONS REVIEWED

PICKWELL ON THE PRAIRIE HORNED LARK.*—This thin volume is apt to prove, in the history of ornithology, one of the first and best fruits of the new life which has been infused into the old science of bird-watching.

It is to be expected that the new understanding of certain principles of bird behavior—if not of their nature or history—will be tested first and most thoroughly in studies of easily-observed birds, and that these studies will dominate the science for a long time. Examples are Selous's colonial sea-birds, Verwey's herons, A. A. Allen's blackbirds, Friedmann's cowbirds, Miller's shrikes, and the present volume. All represent brilliant pioneer opportunities, but none more than the Prairie Horned Lark. A bird with a deep-seated abhorrence of cover is a great ornithological convenience; and one which devotes six months of the year to breeding simply trebles, for the time-being, the life of the ornithologist. When to these practical advantages are added a list of startling peculiarities—the semi-precocial nature of the young, the hopping stage, the towering flight-song, the nest-digging and -paving, the endurance of spectacular weather conditions, the great historical extension of range—the problem takes on prismatic color.

About half the book is devoted to the long section on reproduction. The bird "selects the bleakest barrens available in every locality in which to nest" and through the long season retreats before the single intolerable factor of *verdure*. The season of song in the species extends from mid-January in Kansas for *praticola* to at least August 29, near Great Slave Lake, for *hoyti*. At Evanston, Illinois, for *praticola*, it extended from January 10 to July 14, 1926. "There is probably no other passerine bird that can equal this record." May is the month of most frequent song, and frequency may be affected by weather. The flight-song, strangely ignored in comparison with the related exhibition of the Skylark, is delivered at heights of between 270 and 810 feet, and is responsive to certain obvious stimuli such as the repulse of an invading male, or human disturbance. The author believes the song a proclamation of territory, and not intended for the ears of the female. At Evanston, territory was delimited and fought for by a male on February 7, 1926, and at Ithaca subsequent nestings were found as nearly on the original territories as increasing vegetation would permit. Territories are compressible in size under pressure of numbers, though impalpable boundaries, wider but still definite, exist when competition is lacking.

Both sexes make journeys afield, but feed for most of the time in the territories. Combat is invariably aërial and involves strange rules and conventions. Four February nests are recorded in the literature, and "March nests are the rule from Kansas to Manitoba, from Manitoba to the Atlantic", though with a somewhat low average of successes. This is earlier, in an absolute sense, than even the southern *giraudi* or *chrysolaema*. A temperature of from 40 to 45 degrees Fahrenheit, usually extending for two days, is an initial requirement, but great resistance to ensuing severe weather is shown. "This seems to have more than a little of prescience in it"—namely, "that of more than thirty nests, nearly all should have the protection [usually a tuft of grass] on the west and northwest." But the reviewer must apologize for being led so far into mere sampling of this fine material.

A heavily documented historical section of twenty pages is largely devoted to the story of the northward extension of range, for the most part since 1870, with a parallel, drawn from Gätke and criticised by Naumann, in the problematical recent northern extension of *O. a. flava* in Europe. Both the northern extension of *praticola* and its possible genetic derivation from a southern race are suggested as throwing some light on the grotesquely early breeding—a hypothesis not likely to find favor, especially in the light of other well-supported evidence of a dependence upon temperature rather than upon a rigid physiological cycle, but which we must in fairness admit is hardly more than suggested.

The most interesting parts of the con-

*Transactions of the Academy of Science of St. Louis | Volume XXVII | The Prairie Horned Lark | Gayle B. Pickwell | Issued August, 1931. | Post octavo (125 x 228 mm.), pp. 153 + Table of Contents and Index (unnumbered), Plate I (frontispiece) + Plates II to XXXIV (bound at end), 18 figures and 24 tables in text.

cluding sections on the ecology of the nest site with reference to other birds were the episodes which showed the perfect indifference of the breeding males of *praticola*, in April, to intruding *O. a. alpestris* (birds of the latter race were obviously ignored as belonging physiologically to another world), and the ecological series, passing from the frequenters of the heaviest to those of the scantiest vegetation—Dickcissel, Bobolink, Savannah Sparrow, Meadowlark, Vesper Sparrow, Prairie Horned Lark. The Vesper Sparrow, too, suffers displacement by increase of vegetation, and, "Of all resident birds the Vesper Sparrow is nearest the Prairie Horned Lark ecologically."

Results of compression are evident and leave us sometimes not quite satisfied. There are a few curious omissions. For instance, no direct information, beyond what is implied in the diagrams of territories and in their discussion, is given on the maximum or average number of broods brought off by the subspecies, nor the relation of the old to the new breeding activities. We are not told how the identity of the same pairs on the successive territories at Ithaca was established. Isolated statements, such as "the development of feathers depends entirely upon growth in weight, that is, upon amount of food", or "the age of the young at nest-leaving depends upon the manner and amount of food they have received", suggest the existence of valuable data for the presentation of which no room was found.

Probably no other work is so important in the advancement of the science of birds as such unified and modern studies of behavior and its relationship to the surroundings. The time is not far off when we shall compare such factors throughout our avifauna as readily and as concretely as we compare birdskins today, and build new and hardly less trenchant doctrines on the results. Yet this alone is not enough. The perfect combination will be reached when such work is done *inter-dependently* with laboratory investigations of comparative anatomy and ontogenesis, each bent upon a common, and not merely descriptive, end. Then and only then can we hope to explain, as well as to describe, the peculiarities of the Horned Larks—perhaps even of the Prairie Horned Larks.

It is a pity that a piece of work of such permanent and unquestionable importance should have been marred by slovenly editing. The following are a few of the minor atrocities which reached final printing. "Nutall" twice on page 4; "From a territory such as this a male bird will not leave" on page 50; a printer's jumbling renders a quotation from Sutton incoherent on page 58; "the initiatory temperatures was" on page 62; April 9 should read April 19 on page 73; a "the" is omitted and the phrase "first heard in April 21" occurs, both on page 114; etc., etc.—T. T. McCabe.

THE RIDDLE OF MIGRATION.*—This latest contribution of Rowan's to the study of bird migration is a summarization of his experiments and arguments on the subject. The presentation of new data seems not to have been the object of the book, for the material used has appeared in several shorter papers by the author in various scientific journals. These papers unfortunately are not cited or listed in a bibliography. The chief preliminary paper (Experiments in Bird Migration) I have already reviewed in these columns (Condor, XXXII, 1930, pp. 166-168). The book before us for consideration is written in a style which should make for easy understanding on the part of amateur bird students. The biologically trained ornithologist probably will overlook certain descriptions of the commonplace in his appreciation of the ingenious and thoroughly valuable discussions.

Opening with an esthetic picture of the marvel of northern migration in the section headed "Prologue," the author turns to a matter-of-fact viewpoint in the first chapter which deals with the living bird. Here for the sake of biological background is an interesting running discussion of certain features of avian structure and physiology, with special emphasis on the nervous system, sense organs, powers of flight, and endocrine glands. The large amount and the ease of flight performed by species or individuals that may be entirely non-migratory is emphasized as indicating the potentialities for migration. His contentions that birds are undoubtedly more successful in the air than bats and that the spectacular migrations of birds are unknown to the latter are rather sweeping and are open to criticism.

Under a section on "Environment, Past and Present," he proposes to pay particular attention to the well-known migra-

*The Riddle of Migration, by William Rowan. Baltimore, The Williams and Wilkins Company. 1931. xiv+151 pages, 11 figures in text.

tions of the north. "If we can analyse them here we can apply our findings to migration elsewhere after due allowance for other factors." Perhaps it could be argued that instead one should work in the opposite direction, that is, from the incipient migrations of other regions to the well-marked migrations of the north in order to understand the latter. Important is his attitude, with which I agree, that migration is suited to conditions of the environment as they exist today. Migration in many instances doubtless existed in pre-Pleistocene time under conditions similar to the present, but migration is not dependent today on some set of factors that are not operative now. Migration is maintained and may arise in adaptation to existing environments. (See also Grinnell, Auk, 48, 1931, pp. 22-32.) Certain migrations of shorebirds may best be explained by supposing them to be remnants of adaptations to conditions peculiar to the past, but these cases constitute exceptions to the general rule. Consequently the Pleistocene ice age as a cause of present day migrations is not considered important. This is in line with more recent concepts of the Pleistocene ice which is thought to have been less continuous than once supposed. In considering northern winter environments, food supply and ultra-violet radiation are singled out as being the most important selective factors. Temperature is not important. The possible function of the oil gland in relation to vitamin D storage and ultra-violet radiation is an interesting angle which he introduces. Some discussion is presented of cyclical variation in numbers of animals in the north over periods of several years.

Elimination by natural selection of those individuals or groups failing to migrate from among populations of a species of variable migratory habit preserves stirps composed of migrating individuals. This does not indicate a learning and subsequent inherited habit to migrate but merely a selection of variations in habit which presumably already are heritable. The failure to migrate and the consequent death of Mallards that remain in Alberta during normal winters is cited as a case in point. These remaining Mallards fail to migrate even though the lakes may entirely freeze over. The assumption seems to be that they have not inherited the migratory response mechanism which most other Mallards of the north possess. They are not able to originate migration in response to the immediate situation, that is, the freezing of the lakes. Selection in such cases which involve food supply he feels is obvious; it may not be so clearly demonstrated in instances of ultra-violet deficiency. Whether the original ability to migrate was acquired in a Lamarckian sense or not must be left unanswered. Rowan feels that some time in the distant past the ability to migrate or wander was developed in bird ancestry, probably in connection with increasing mobility. This general heritable aptitude to migrate or wander some birds now make use of whereas others do not.

Set apart from the discussions of the evolution and utility of migration is a consideration of the mechanism which is in operation in the control of migration each year. This is shown to be variation in day length. Variation in day length affects the activity of the bird which in turn induces increases or decreases in the gonads. During certain stages in the process of increase and decrease interstitial tissue becomes prominently developed in the gonads. This tissue is known to produce sex hormones. In Juncos the appearance of interstitial tissue coincides with the time of migration and it is assumed that a hormone is produced by the tissue which induces the bird to migrate. Temperature and other weather conditions have either limited or no effect on the gonad-migration rhythm. This concept of the mechanism of migration is the same as that which I previously reviewed. Since then some additional evidence has been brought out in experiments with crows, but the migration hormone must still be regarded as an assumption, as Rowan himself admits. I do not feel that the possibility of simultaneous and independent action of the bird's activity, or its activity controlled by day length, upon the nervous system and the gonads is as yet ruled out. There is only circumstantial evidence that the interstitial tissue of the gonads and its hormones stand in causal relation to the migration rhythm.

The attitude of Rowan in advancing these theories in this book is admirable and, it seems to me, much less dogmatic than in certain earlier papers. He says: "The arguments on which they (the theories) are based apparently fit the facts, but it must be borne in mind that

any theory that is new must remain subject to correction until fully substantiated by repeated effort and the critical investigations of fellow workers looking at the problem from some other angle. . . . Experimental biology, however, is peculiar in this respect, that the investigator must of necessity have a preconceived theory upon which to work."

Rowan's concept of the mechanism of migration gains weight with each additional experiment. The present volume brings under one cover his work to date in this field, which work is the most searching and thorough investigation of its kind. In the foregoing discussion I have freely transcribed into my own words only a very few of the author's ideas. A careful reading and study of the book is to be highly recommended to all students of birds.—ALDEN H. MILLER, *January 23, 1931.*

MINUTES OF COOPER CLUB MEETINGS

SOUTHERN DIVISION

NOVEMBER.—The regular meeting of the Cooper Ornithological Club, Southern Division, was called to order by Vice-President Harold Michener, at the Los Angeles Museum, Exposition Park, Los Angeles, at 8 p. m., Tuesday, November 24, 1931. Minutes of the October meetings of both divisions were read, and those of the Southern Division were approved.

Applications for membership were read as follows: Marie Everett, Randsburg, California, proposed by Loye H. Miller; Miss Mary E. Fancolly, Apt. 1, 819 North First Ave., Tucson, Arizona, proposed by Walter P. Taylor; and Miss Elizabeth Hager, 2322 Observatory Ave., Los Angeles, proposed by Berry Campbell.

The matter of a delegate to the Fifth Pacific Science Congress having been held over from the last meeting, the secretary stated that Dr. Miller had received a later announcement that the Congress has been postponed for one year.

A letter was read from the Secretary, Pacific Division, A. A. A. S., requesting that representatives be appointed to meet with the Affiliation Committee in San Francisco in January. Harold Michener stated that it is customary in such cases to ask any representative who may be appointed by the Northern Division to represent the Southern Division also.

The following resolution was read:

Whereas, The all-wise power that controls the destinies of men has seen fit to withdraw from our companionship our friend and fellow worker, John Eugene Law; and

Whereas, We realize that for many years our Club has benefited from his unselfish and devoted efforts for its betterment, and that the science of ornithology has been materially advanced through his earnest endeavors to solve many of its complex problems; then be it

Resolved, That we, the members of the Southern Division of the Cooper Ornithological Club, hereby express our appreciation of the great loss that has come to us individually, to our Club, and to the science of ornithology at large; and, be it further

Resolved, That the Secretary be instructed to extend our sympathy to the bereaved family of our friend.

It was moved by Dr. Miller that the resolution be adopted, and that a standing vote be taken. The motion was seconded by Chester Lamb and was carried unanimously. A short note from Mrs. Laura B. Law, expressing her appreciation of the flowers sent to Mr. Law's funeral by the Southern Division, was read.

As there was no regular program the meeting was opened for discussion. Chester Lamb told of some of his recent experiences collecting in Lower California. Dr. Miller reported the finding of fossil bird remains in a new horizon near the breakwater at San Pedro, and of having recently seen a Summer Tanager in Los Angeles. Chester Lamb spoke of having recently seen the Great-tailed Grackle in Las Cruces, New Mexico. Dr. Miller told of the Mearns Quail in southern Arizona hatching young in late August after the rains had brought on spring-like conditions.

J. W. Sugden was present and told how some of the Cooper Club members in Salt Lake City still meet although the Inter-Mountain Chapter has not functioned for a number of years. He also reported the Eastern Kingbird, the Arkansas Kingbird and the Cassin Kingbird to be found nesting within a mile of each other in Utah. He also gave some interesting observations on the nesting waders of the Salt Lake region.

Chester Lamb reported a man at Santa Rosalía, Lower California, who claimed to have taken 11,000 Heermann Gull eggs from the islands in the Gulf of California, and told of others who had taken 6,000 more. The eggs are sold for food.

J. W. Sugden said that the California Gull, the sacred bird of Utah, has been reported to be a menace to the cherry crop in some sections of Utah, picking the ripe fruit from the trees as they fly past. After some further discussion the meeting adjourned.—JOHN MCB. ROBERTSON, *Secretary.*

DECEMBER.—The December meeting of the Southern Division of the Cooper Ornithological Club was called to order at the Los Angeles Museum, Exposition Park, Los Angeles, at 8 p. m., Tuesday, December 29, 1931, with about twenty-five members and friends present. The minutes of the November meeting of the Southern Division were read and approved; the minutes of the November meeting of the Northern Division were read.

Applications for membership were received as follows: Louis A. Sanford, 6518 Moore Drive, Los Angeles, proposed by George G. Cantwell; and Lucien B. Howsley, 715 Bankers Bldg., 629 South Hill Street, Los Angeles, proposed by W. Lee Chambers.

A copy of Jean M. Linsdale's report on conservation, adopted by the Northern Division at its November meeting, was read by the Secretary. It was moved by Dr. Loye Miller that the Southern Division go on record as endorsing the action of the Northern Division in adopting this report. The motion was seconded by Dr. Rich, and was carried.

A letter addressed to W. Lee Chambers asking about the date of the coming Annual Meeting was read. Dr. Loye Miller stated that the last half of April seems to be the most suitable time for this meeting, and after some discussion, it was moved by Chambers that the Southern Division suggest to the Board of Governors that the 1932 Annual Meeting be held between the dates of April 18 and 25. The motion was seconded by Dr. Loye Miller and was carried.

It being time to consider the election of officers for the year 1932, the chair appointed W. Lee Chambers, Dr. Loye Miller, and J. S. Appleton as a nominating committee to report at the January meeting.

Alden H. Miller was called on for remarks on recent field experiences, and after expressing his appreciation of the action of the Southern Division in endorsing the stand of the Northern Division on conservation, he told briefly of the collecting trip taken by himself and wife last summer. Their object was to study the subspecies of Juncos of the Rocky Mountain area, and they camped in Junco breeding grounds from southern Arizona to northern Utah, and found some interesting problems in subspecific relationships.

J. S. Appleton told of a Horned Owl recently killed by his son at Santa Susana, as it was killing his pigeons. Dr. Loye Miller spoke of the scarcity of owls in December in parts of southern Arizona where they were abundant in September. The question was raised as to the origin of the Chinese Spotted Dove in Los Angeles, and it developed that it has been in the Hollywood area for at least fifteen years, but no one present knew who liberated the original birds. After further comment on southern Arizona birds by Berry Campbell and Dr. Miller, the meeting adjourned.—JOHN MCB. ROBERTSON, *Secretary.*

NORTHERN DIVISION

NOVEMBER.—The regular meeting of the Northern Division of the Cooper Ornithological Club was called to order by President Wright on Tuesday evening, November 24, 1931, at 8:00 p. m., with about 125 members and guests present. Minutes of the Northern Division for October were read and approved. Minutes of the Southern Division for October were read. Proposals of new names for membership were: Patrick W. Martin, Tempe, Courchan Station, Vancouver Island, B. C., by E. L. Sumner, Sr., through the Western Bird-banding Association, and Alfred P. Redington, 900 State Street, Santa Barbara, California, by Joseph Mailliard.

Mr. H. S. Swarth brought before the meeting the announcement of the death of John Eugene Law, a member of the Club since 1900 and for nineteen years one of the business managers. Mr. Swarth moved that a committee be appointed by the Chair to draw up resolutions of sympathy to be spread upon the minutes and a copy forwarded to Mrs. Law. The motion was duly seconded and unanimously passed by a rising vote. President Wright appointed Mr. Swarth, Joseph Dixon, and E. L. Sumner, Jr.

Mrs. Allen announced that many Varied Thrushes are now to be seen in Strawberry Canyon. Leslie Hawkins reported having seen forty-five Canada Geese on San Pablo Reservoir on November 22, and Brighton C. Cain told of having seen a Spotted Owl in Marin County on the same date. Dr. Gayle Pickwell described a wintering colony of White-throated Swifts found in Sycamore Canyon near San Jose. At least fifty of the birds were seen entering a two-inch crevice in the rocky walls a few moments before sunset and many

more were heard chattering in the recesses of the fissure. Mr. Swarth told of watching a Wilson Snipe at Port Rupert and stated that the bird allowed him to approach within four feet, as it stood with eyes almost shut. Mr. E. L. Sumner, Sr., reported baiting banding traps in Strawberry Canyon with hulled barley in place of the chick-feed usually used, in order to determine whether the birds had a preference for one or the other. Forty-one birds were caught in barley-baited traps during the time that forty-eight were caught in traps baited with chick-feed.

The topic of the evening was "Urgent Problems in Bird Protection" and Mr. John G. Tyler of Fresno opened the question with a motion that Mr. Jean M. Linsdale's report [see below] on conservation be taken from the table where it was placed at the October meeting of the Northern Division by a vote of 9 to 6. The motion was duly seconded and passed. President Wright then read Mr. Linsdale's report and the report of the special committee, consisting of Mr. Follett and Mr. Cain, which was appointed to consider it. He then invited discussion of the report. During the hour which followed, discussion was participated in by Alden Miller, W. C. Jacobsen, T. T. McCabe, Paul Bunker, E. R. Hall, Donald McLean, B. W. Evermann, Joseph Dixon, Sumner Brooks and Ira Gabrielson. Mr. Grinnell then called for the question (adoption of the Linsdale report) and the Chair asked for a rising vote. Of the 125 persons present 61 voted, 52 in the affirmative and 9 in the negative. The Linsdale report is appended to these minutes.

Adjourned.—HILDA W. GRINNELL, *Secretary.*

REPORT ON CONSERVATION TO THE NORTHERN DIVISION, COOPER ORNITHOLOGICAL CLUB, SEPTEMBER 24, 1931

In 1929 the undersigned was asked to investigate problems of bird conservation in California, for the Northern Division of the Cooper Ornithological Club. Two reports have been presented and have received the approval of the Club. The following brief outline is intended to be a further report upon one phase of the situation as it has developed recently.

The welfare of the bird life of California has had no more serious threat than that which accompanies the quickening movement on the part of agricultural interests for the artificial "control" of all animals and plants which appear in any degree to interfere with, or even not to contribute to, immediate profits from the land. The phase of this movement which most concerns this Club is the imminent plan to destroy bird life, this in response to the complaints of farmers and fruit-growers.

The progress of farming has reached a point where there is no room left on the land for wild birds and mammals, *if* every bit of the available energy is to be turned to the production of crops. The question must be decided: Do we want the largest possible production of planted crops with no wild animals except those which persist as pests, or do we want a stabilized crop yield with allowance for the support of representatives of our native fauna? Study of all phases of this problem shows that the interests of the whole community would be served best if provision were made for the maintenance of a substantial portion of the native fauna on the land. The situation is not to be thought of in terms of but one season or even of a short series of years.

Several agencies are teaching and even forcing people to remove the native animals from their land. This is taking place against the wishes of a large proportion, possibly a majority, of the rural population. But the movement for "pest" control is rapidly reducing the number of persons who *favor* the presence of wild animals, and increasing the number who consider every plant and animal, not domesticated, to be a *pest*—to be removed, no matter how great the cost.

The use of poison to kill animals not wanted on the land provides so many opportunities for unnecessary drafts upon wild life that I believe it not justified in California to the extent it has been used within recent years or to the extent desired by a large proportion of agricultural workers.

In thinking of this problem it is necessary to keep in mind the characteristics of each of the three important uses that are being, or are likely to be, made of poison in the state. These are: (1) To poison rodents, chiefly the California Ground Squirrel; (2) to poison meat-eating mammals, chiefly the coyote; (3) to poison birds, chiefly the blackbirds and finches. Care should be taken not to confuse the conditions concerned in each. Statements made about one do not neces-

sarily apply to the others. They should be examined separately.

Our immediate concern is with the destruction of birds by poison. My enquiries thus far point to the placing of poison whether intended for birds or rodents on farmlands as the most serious of the preventable agencies which threaten the welfare of birds in general in California. The threat includes not only the direct losses in numbers of birds, which is perhaps least important, but the further disturbance of adjustments of native animals to conditions of cultivation, and the replacement of sentiment in favor of birds with a determined prejudice against them, which prejudice permits or abets their removal on slight pretexts.

The conditions outlined above bring me to recommend:

That the Northern Division of the Cooper Ornithological Club reaffirm as one of its major objectives "the conservation of birds and wild-life in general, for the sake of the future."

That members of the Club in making effort to acquaint themselves with various phases of the pest control movement, take care that they seek information from unprejudiced sources.

That the Club urge the proper authorities to make a careful canvass of all the cultivated areas in California, to determine the extent of the supposed harm by birds—the crops, the birds, the seasons concerned. (This has not been done; nor as far as can be determined, is it contemplated. So far, the whole effort to control birds has been in response to complaints which, though numerous, represent only a small per cent of the whole area farmed in the state.)

That the Club urge that poison be not used to kill native wild birds, or, to the extent or manner practised within the past four years, to kill native wild mammals in California, and especially that no new projects be undertaken which involve the use of poison in the regulation of animal numbers.

JEAN M. LINSDALE.

DECEMBER.—The December meeting of the Northern Division of the Cooper Ornithological Club was held on December 17, 1931, at 8:00 p. m., in Room 2003, Life Sciences Building, Berkeley, with about 65 members and guests present. President Wright occupied the Chair. Minutes of the Northern Division for November were read and aproved. Minutes of the Southern Division for November were read. Miss Margaret Sumner, Kenwood Hall, 6367 Wilshire Boulevard, Los Angeles, California, was proposed for membership through the Western Bird-banding Association by Mr. E. L. Sumner, Senior.

Mr. Swarth, chairman of the committee appointed in November, read the following resolutions:

Whereas, In the death of our Honorary Member, J. Eugene Law, the Cooper Ornithological Club has lost a devoted adherent, one who for many years past has had the Club's welfare prominent in his thoughts and a prime object in his activities; and

Whereas, Our chosen study, of Ornithology, has similarly lost an ardent and accomplished follower, of whose past accomplishments we are proud and whose continued investigations would have redounded to the credit of the Club; and

Whereas, Our members, individually, are keenly conscious of personal loss in the departure of a valued friend and companion; therefore,

Be it resolved, That we record here our sense of sorrow at the loss we have sustained, and that we transmit to Mrs. Law a copy of these resolutions, as also our sympathy with her in the still deeper sorrow that is her portion.

The motion to adopt these resolutions was made by Mr. Swarth, duly seconded and unanimously carried.

A letter from Professor Luck, secretary of the Pacific Division of the American Association for the Advancement of Science was read. Professor Luck requested the presence of two delegates from the Northern Division at the meeting of the Affiliations Committee to be held in San Francisco on January 8, 1932. President Wright requested Dr. Evermann and Dr. Ritter to serve in this capacity and asked them to represent the Club as a whole, following a suggestion to this end from the Southern Division. The Chair announced the appointment of a committee to report nominations of officers for 1932 at the January meeting, the personnel being Mrs. Mead, Alden H. Miller, and Mrs. Allen, chairman.

An illustrated talk upon the "Life Cycle of the Water Ouzel" was then given by Mr. Bert Harwell of the Yosemite Museum. Mr. Harwell reported Ouzels as being present in all five life-zones of Yosemite Park and gave many interesting facts on nest-building, food, care of the young, and local distribution. His enthusiasm for the Ouzel met with a clearly apparent, sympathetic response from his audience.

Adjourned.—HILDA W. GRINNELL, *Secretary.*

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